SCIENCE.

FRIDAY, SEPTEMBER 12, 1884.

COMMENT AND CRITICISM.

The American association for the advancement of science borrowed its constitution, in large measure, from the British. Yet, while it is evident in the nature of things that the same rules cannot answer for two countries differing so widely in geographical extent, one weakness of the American, as compared with the British society, lies in its lack of an efficient organization in the interim between two meetings, and the necessity that the non-permanent members of the standing committee should be chosen from and by the members present at one of the annual gatherings. This deficiency has been emphasized by the visit of the British association on this side of the water, and by the proposal for an international association of some sort.

This leads us to draw the attention of those interested to one or two features of the recent Montreal meeting, which might well be adopted by the American association, and would require no alteration of the constitution. One is the grouping of papers in each section, so that those of a similar character are read together, eliciting a better discussion, freer from discursiveness, and at less cost of time; another is fixing set subjects for discussion on some topics of interest, to be opened by designated members; a third is the daily disposal of the entire schedule, no matter how much the papers have to be abbreviated or the session prolonged, so that each day's programme is fresh. The most important of all is the appropriation of grants of money to committees for special scientific work during the year, the grants this year amounting to over £1500.

The growth of the American association, during the past five years, warrants the belief that such grants are entirely within its disposal, if it will simply reverse its plan of printing papers in full. We believe that only five of the numerous papers read at Montreal are to be printed *in extenso;* such papers having to be recommended by the sectional committees, and approved by the general committee. In our own association, the matter is completely within the control of the standing committee, which, by adopting a similar policy, might soon bring the association into possession of a permanent fund of fifty thousand dollars, — such as the British association now enjoys, — instead of leaving it to fulfil but half its mission on its paltry investment of a couple of thousand dollars. At present, the American association is expending more than four thousand dollars a year in printing; while the British association, with twice the membership, and an average presentation of twice as many papers, prints no bulkier a volume, and less than half of it is made up of members' papers. The avenues of publication in America are now ample enough for all papers of permanent value.

It has been justly held, that the meeting of the British association in Canada would produce a direct stimulus to science in the dominion. The association itself has evidently determined that it shall. Welcomed with the utmost cordiality, fostered by the government, and receiving the marked attention of the governor-general, it has raised, among its own members, a science-scholarship fund for McGill University, — probably to be devoted to civil engineering, — has been the occasion of a gift of fifty thousand dollars for a public library in Montreal, and has passed a series of resolutions pointedly calling the attention of the Canadian government to two important duties to science and humanity which it has hitherto neglected, — a proper system of tidal observation along its extended coasts, for the benefit of navigation; and systematic researches upon the native tribes of half a continent.

ETHNIC problems have a natural interest for the American people. Their great task is to fuse together the life of many lands, — to bring political and social union out of the widest diversities that the races of men afford. They follow a true instinct in giving time and public money to such problems. The bureau of ethnology is doing an admirable work in gathering the history of our departing aborigines. There is, however, another field of labor, — one not yet fairly entered on, either by private students or by the ordered phalanxes that are marshalled in the cause of science by the bureaus of the federal government. As the indigenous savages were forced towards the setting sun by the plough-driving Aryans, the shore was crossed by another savage race, the African, that has come to stay for all time in our fields.

There can be no question that the African in the United States presents us with the greatest and most interesting experiment that has ever been tried by civilized man upon a lower people. Around this race have gathered a host of problems of the utmost importance to pure science, and of infinite interest in that field of nature called sociology, into which science is with such difficulty making a slow and blundering way. Out of the very numerous inquiries that should be made in this field we may note the following, that are at the moment, perhaps, the most important because they concern matters that need to be studied at once. *First* among these is the question of the origin of our American negroes. There is a great deal that still can be gathered concerning this question. No close observer of the negro race in this country can fail to have noticed the wide diversity of type masked behind the deceiving uniformity of hue. *Second*, we have the problem of the physical and mental change that has come over this people since their removal to America. *Third*, the effects of climate in different parts of the United States upon these black races, — effects on shape, liability to disease, longevity, etc. What to do with and for the negro, and how to do it, is the question of all questions most immediately and imperatively before us. We best begin to deal with it by making a scientific study of him.

LETTERS TO THE EDITOR.

**** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The initiation of deep-sea dredging.

In a recent number of *Science* (July 18), Mr. Rathbun is rather severe upon European naturalists for their supposed ignorance of the fact that the Gulf-Stream dredgings carried on by the Corwin, under the superintendence of the late Mr. Pourtalès, were commenced in 1867, the year before the first British expedition in the Lightning; and he speaks of Mr. Pourtalès' report of December, 1867, as having been 'utterly ignored' by European writers.

It is quite true that no reference was made to this report in the historical account of the subject which formed part of the preliminary report of the dredging operations of the Lightning, presented to the Royal society by Dr. Carpenter on Dec. 17, 1868; for the bulletin of the Museum of comparative zoölogy, which contained Pourtalès' report, had not then reached him. The correspondence between Dr. Carpenter and Sir Wyville Thomson, which led to the cruise of the Lightning (published as an appendix to Dr. Carpenter's report), was carried on in entire ignorance of the fact that Pourtalès had dredged down to a depth of three hundred and fifty fathoms a twelvemonth before. In fact, it was only after their return in September, 1868, that they heard for the first time of the work done by Mr. Pourtalès in May of that and of the previous year. But a short account of it, received from Prof. A. Agassiz, was quoted by Dr. Carpenter; and reference was given to a fuller notice of Mr. Pourtalès' results in Silliman's journal for November, 1868.

It will be seen, therefore, that Dr. Carpenter, far from ignoring the researches of Mr. Pourtalès in the Corwin, fully recognized their priority to those carried on in the Lightning during the autumn of 1868. He could not well refer to a document, which, though published a year previously, had not yet come into the hands of British naturalists, and consequently could not receive from them the credit which Mr. Rathbun says has been denied it. But Mr. Pourtalès' dredgings were noticed in the same number of the proceedings of the Royal society as were those of the Lightning; and I do not well see how their value could have been more fully recognized, considering what was then known about them in this country.

I freely admit, however, that in 'The depths of the sea,' the book to which Mr. Rathbun so pointedly refers (though without naming it), it is stated that the dredgings of Mr. Pourtalès were 'commenced' in 1868. This is one of several minor inaccuracies which are unfortunately to be found scattered through the work; and, however much they are to be regretted, it must be remembered that at the time it was written the author was in bad health, with his time fully occupied by his professorial duties, and by the preparations for the cruise of the Challenger, which commenced almost before the book was in the hands of the public. In fact, the later chapters, which contain the erroneous reference to the date of

Mr. Pourtalès' first dredgings, were written under very considerable difficulties, as I well remember hearing from the author himself. But the 'priority in scientific research,' which Mr. Rathbun claims for Pourtalès' work, had been accorded to it four years previously, at the earliest possible opportunity, in the Proceedings of the Royal society. So far as I know, this honor has never been 'denied' to one who would have been the last to claim it for himself. I fully admit, however, that the date of his earlier work has been incorrectly given in certain popular accounts of the subject; but this was done accidentally, and without the slightest intention of appropriating any credit for the work of British naturalists which was justly due elsewhere, as will be evident from what I have said already.

P. HERBERT CARPENTER.

Eton college, Windsor, Eng.,
Aug. 11.

The 'bassalian fauna;' 'Pentacrinus asteriscus.'

I notice that Mr. Gill has "recently proposed the name 'bassalian realm' for the collective deep-sea faunas." I do not know whether it is proposed to define this name more strictly by assigning to it any particular bathymetrical limits; but it may be well to notice, that, in his presidential address to the biological section of the British association at Plymouth in 1877, Mr. Gwyn Jeffreys suggested the use of the name "'benthal' (from the Homeric word βένθος, signifying the depths of the sea) for depths of one thousand fathoms and more," while retaining the term 'abyssal' for depths down to one thousand fathoms.

There is another point to which I have long thought of directing the attention of the readers of *Science*, and I therefore take this opportunity of doing so.

The surveys of Hayden, Wheeler, and others, in Utah, Idaho, and Wyoming, have revealed the very wide distribution, in beds of Jurassic age, of a crinoid which has been called Pentacrinus asteriscus. Nothing is known of this form but a number of stem-joints (I speak under correction, and shall be pleased to hear that I am wrong); but most of the figures of these joints which I have seen (e.g., that given by White in the paleontology of Wheeler's survey) seem to me to indicate that the type should be referred to Extracrinus rather than to Pentacrinus. The essential characters of the stem-joints of Extracrinus are well shown in plate liii. of Buckland's 'Geology and mineralogy,' figs. 9–13; on tab. 101 of Quenstedt's 'Encriniden,' especially figs. 24, 27, 33, and 37; and also on plate xii. of the Austins' 'Monograph of recent and fossil crinoids.' The five interradial petals are quite narrow, and much less distinctly oval than in Pentacrinus, sometimes becoming almost linear, with rounded outer ends. The interpetaloid spaces are plain, and devoid of sculpture; while the markings at the sides of the petals are much more delicate than in Pentacrinus, having more the character of striae or crenulation than of coarse ridges. They are also much more numerous than in Pentacrinus, and are limited to the sides of the petals, not reaching the outer edge of the joint-face. Under these circumstances, I suspect that it is to Extracrinus, and not to Pentacrinus, that we must refer the joints which were described by Meek and Hayden as having lance, oval, petaloid areas, "bounded by rather narrow, slightly elevated, transversely crenulate margins."

Extracrinus was proposed by the Austins for the two well-known liassic fossils, Pentacrinus briareus and P. subangularis; but recent investigations have shown that the genus extends up into the great oolite (Bathonien) of Britain, France, and Switzerland. I have no knowledge, however, of any triassic species of Extracrinus; though Pentacrinus is well represented in the St. Cassian beds, and has been found associated with Encrinus in the 'wellenkalk' of Würtemberg.

It is therefore interesting to find that the triassic form of Pentacrinus asteriscus, which was obtained by the fortieth parallel survey from the Dun Glen limestone and the Pah Ute range, differs from the Jurassic specimens found in south-east Idaho and western Wyoming, almost precisely in those points which distinguish Pentacrinus from Extracrinus. According to Hall and Whitfield, the chief distinction of the triassic forms lies "in the more obtuse points of the star, and the filling-up of the angles between the points, and also in the broader form of the elliptical figures on the articulating surfaces of the disks." They suggest that the differences may possibly be of specific value; but, having carefully studied a large variety of stem-joints of Pentacrinidae, both recent and fossil, I am inclined to go farther, and to suspect that the triassic type may belong to Pentacrinus, but the Jurassic form to Extracrinus.

The two genera differ very considerably in the characters of the calyx and arms, as will be fully explained in the report on the Pentacrinidae dredged by the Challenger and the Blake, which will appear in the course of the winter. But, in the mean time, I shall be most grateful for any information respecting Pentacrinus asteriscus, in addition to that which has been already made public; and I need not say that I should much like to have the opportunity of making a personal examination, both of the triassic and the Jurassic specimens. P. HERBERT CARPENTER.

Eton college, Windsor, Eng.,
Aug. 11.

Points on lightning-rods.

The following passage occurs in J. E. H. Gordon's excellent "Physical treatise on electricity and magnetism," vol. i. p. 24: "It was held that the knobs [on the ends of lightning-rods] must be most efficacious, because the lightning was seen to strike them, and never struck the points. The fact that a point prevents the lightning from ever striking at all was not known."

This is not true. The highest rod on my house is some fifteen feet above the others, and about thirty feet higher than the surrounding buildings; and yet, notwithstanding the fact that it is tipped with a brush of five points, it was struck a few years ago. The points are gilded iron, and the topmost one was melted into a ball about one-eighth of an inch in diameter. The rods are all connected by horizontal pieces held about three inches from the tin roof by glass insulators, after the fashion of ignorant lightning-rod agents. The neighbors say that the sparks flew so thickly between the rods and the roof, as to resemble a sheet of flame. The shock was, singularly enough, so slight that it is doubtful whether it was due to the electrical discharge, or the deafening crash of thunder that instantly followed the splitting sound of the spark. A. B. PORTER.

Indianapolis, Aug. 23.

Photographs of the interior of a coal-mine.

One of the most interesting enterprises to which the preparations for the New Orleans exposition have given rise is the successful attempt to photograph the interior of a coal-mine in Pennsylvania. The mine selected for the experiment was the Kohinoor colliery at Shenandoah, operated by the Philadelphia coal and iron company, from whose representatives all necessary facilities were obtained.

The experiment was conceived of, and successfully carried out, by Mr. James Temple Brown, who was sent out from the metallurgical department of the National museum to collect specimens illustrative of the coal industry. An attempt was first made to photograph by the aid of magnesium light, but the results proved unsatisfactory. The Arnoux electric-light company then volunteered to supply an electric plant, and to erect and take charge of it gratuitously. The five negatives obtained by the use of this light were highly satisfactory, and show some features of coal-mines which probably have not hitherto been seen by scientific men, nor, indeed, by miners themselves, whose feeble lamps give them only a glimpse of the immediate surroundings.

The photographs will be enlarged, and exhibited at the New Orleans exposition. Whatever credit attaches to this somewhat novel undertaking is due primarily to the generous encouragement of the director of the museum, and to the thoughtfulness and energy of Mr. Brown. The representatives of the Philadelphia coal and iron company very kindly gave the matter their personal attention, and the photographer employed for the work labored enthusiastically for the results obtained.

F. W. TRUE.

U. S. National Museum, Sept. 5.

ELECTRICAL TESTING ESTABLISHMENTS.

THE *Electrical review* seconds the suggestion of the *Engineer*, that an 'electrical testing establishment' be founded in England, where any ambitious inventor may find the apparatus and conveniences which he may need for a proper testing and perfecting of his ideas. The *Review* calls attention to the impossibility of a poor man, however ingenious he may be, being able to work upon any improvement in cable telegraphy, as at least an artificial cable must be at his command, — a necessity which would cost him several thousand dollars. In the same way with experiments on electric lamps: the cost of the necessary plant is very considerable, and the amount required for supplies to be used in constant trials is by no means to be neglected.

The founding of such an establishment for the aid of inventors has been suggested by several of the successful members of the class in America, but has not, we believe, been carried out. There would, at the start, be the difficulty of deciding as to the worthiness of any scheme which might be brought forward for development. The inventor is necessarily an enthusiast, and an extremely fickle being, who would come in one morning all aglow for a new form to be given the carbon filament in an incandescent lamp, and the next would have nothing of lamps, but would earnestly urge some peculiar construction of telephone-cable to get rid of the 'cross-talk.' This constant jumping, accompanied by the necessary amount of perseverance, leads him finally to some goal, but at the same time makes him an obnoxious companion to the steady-going workman who must needs follow him, nothing being more discouraging to an artificer than to see the results of his one day's work overthrown on the next.

It may be urged, that the man with capacity for improving the methods of the world's work will sooner or later, but surely, push himself forward into a position where he can help himself through a connection with some rich telegraph, electric-light, or manufacturing company, where his powers will have full play, and his suggestions be listened to and put in effect. It should also be considered whether, in establishing any 'helping-hand' arrangement, the principal or only result would be to assist those for a time who give promise of valuable development, but who are lacking in the strong fibre necessary for successful accomplishment. Notwithstanding all objections, it may appear to some that the possibility of enabling some one worthy man to bring his work to perfection ten, twenty, or thirty years before he could if left to his own unaided resources, would justify the expenditure of considerable sums on what would be found to be the chaff of inventions. What the result might be, is very difficult to say. There might be some very successful work done in such a laboratory, properly guarded, and where the applicants were kept as constantly as might be to their purpose: there certainly would be a vast number of cranks knocking at the door.

The editorial in the *Electrical review* brought out a response from one of the 'electrical schools' of England, which shows the result of the trial of such a method of aiding inventors, although a free use of the laboratories could not be offered. In this reply it is stated that the school has for several years openly offered the facilities of its laboratories to any inventor who may come forward, and wish such facilities to aid him in perfecting his work; and that as yet they have received two applications, both of which were withdrawn on account of the remuneration which the school felt called upon to ask. One of the applicants was a cable company, and considered five shillings a day too much for the use of the very extensive apparatus required; and the other looked upon five pounds as excessive for the use of power and a dynamo, with skilled superintendence and advice.

As the most feasible solution, for the present, of the question, how to advance the uses of electricity, many of our large telegraph, telephone, and electric-light companies have established testing-laboratories for the use of their employees, and give regular employment to professional inventors whose researches are directed by the officers of the company; but little is done in these laboratories to promote research by persons not connected with the companies themselves. Our universities and technological schools, in many cases, possess well-equipped physical laboratories, containing electrical testing-apparatus for the use of the students. These laboratories exist for the purpose of promoting research, and might, under suitable restrictions, be thrown open to inventors as well as to students.

However the difficulty is to be met, it is undoubtedly the case, that research looking to the utilization of electricity as a motive power and as a source of light is fettered and hindered by the expense of the apparatus required. If a special laboratory, to be under the direction of suitable persons, could be established in this country for the promotion of electrical research, especially for research in those branches that necessitate the employment of expensive apparatus, invention in these branches would be stimulated, and the whole community would be the gainer. In France the profits of the late International electrical exhibition have been devoted to the establishment of an electrical laboratory. Perhaps the managers of the forthcoming electrical exhibition in Philadelphia may take the hint.

AMERICAN APPLIANCES FOR DEEP-SEA INVESTIGATION. — TRAWLS AND TANGLES.

Beam-trawls.

THE beam-trawls designed for zoölogical collecting have usually been patterned closely after those employed by the English fishermen, and in this form are well adapted for moderate depths of water. In fact, the only objection to their use in great depths is their liability to capsize while being lowered, often causing them to land upon the bottom wrong side up. They were first employed on this coast by the fish-commission in 1871; and the earliest records of their use by the English, in deep water at least, are given in the Challenger narrative (beginning in 1873), no reference being made to the subject of beam-trawls in the account of the voyages of the Lightning and Porcupine. In all the exploring-work of the fish-commission, the beam-trawls have been used quite as frequently as the dredges; the trawling-results being far richer as to the larger forms of life, and including immense numbers of fishes which could never be obtained by the dredge, and would otherwise have remained undiscovered.

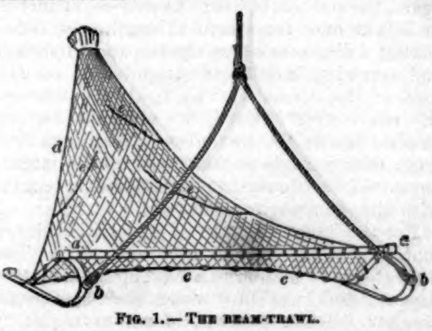

FIG. 1.—THE BEAM-TRAWL.

As is known to most naturalists, the beam-trawl (fig. 1) consists of a large, tapering, bag-like net, which is dragged over the bottom, mouth forwards, to entrap such fish as live close to the ground. The mouth is held open by a long beam, generally of wood, supported upon iron runners; and there are one or more inner, funnel-shaped traps, to prevent the escape of fish after they have entered. The nets are sometimes very large, and the beams often measure forty-five or fifty feet in length. The lower side of the mouth of the net, which is leaded, hangs loose, so as to drag over the ground in a deep backward curve. It does not dig into the bottom, but simply scoops into its capacious mouth every loose object lying in its course. Large quantities of soft sand and mud are, however, often taken up.

In adapting the fishermen's trawl for zoölogical work, a few modifications have been made, mainly as regards size and the materials used in its construction. For small trawls a beam of iron gas-pipe is now preferred by the fish-commission to wood, as being more durable, less bulky, and less liable to injury from pressure in deep water; the defect of wooden beams, in the latter respect, having seriously inconvenienced the deep-sea trawling operations of the Challenger.

The different sizes of trawls employed vary, in the length of beam, from seven and a half to eighteen feet, a wooden beam being used for the latter size only. With an eleven-foot beam, the runners measure twenty-eight inches in height and four feet in length, the beam having a diameter of two inches and a quarter, and screwing into brass strap bands on the tops of the runners. The openings through the runners are closed by netting. In the smaller trawls the net is about eighteen feet long, with a single pocket, and, in the larger, measure from twenty-five to forty feet in length, with either one or two pockets.

For the greater depths of water, the eleven and fifteen feet beams are preferred. The largest size is seldom used, except in moderate depths; and in shallow water, the Otter trawl, another English pattern, is not unfrequently employed.

The common beam-trawl has been used successfully by the fish-commission in all depths down to 2,949 fathoms, the latter indicating the deepest trawling-station on record for the Atlantic Ocean. The Challenger trawled to a depth of 2,650 fathoms in nearly the same locality, but in the Pacific Ocean made several successful casts in more than 3,000 fathoms, the trawl they had having been of about the same size and pattern as the American.

The method of attaching the bridle in the Challenger trawl was similar to that afterwards adopted for the Blake trawl, the bridle ropes being made very long, and extending along the sides of the net to its extremity, with lashings to the runners on each side, and to the hinder end of the bag. The object of this arrangement of the bridle was not stated by Sir Wyville Thomson; but it was presumably to allow the forward fastenings to break, in case of fouling, and permit of the net being hauled up hind-end first.

The Blake trawl.

The objection above raised to the use of the ordinary beam-trawl in deep water suggested to the officers of the Blake dredging-party, in the winter of 1877–78, the construction of a reversible trawl, having in this respect all the advantages of the naturalists' dredge. This pattern, termed the 'Blake trawl,' or 'double beam-trawl,' bears the same relation to the fishermen's beam-trawl as does the naturalists' dredge to the oyster-dredge; the changes in both cases being demanded by the necessity of working with greater precision in deep water, where the loss of time occasioned by the use of ill-suited appliances cannot well be afforded.

FIG. 2.—THE BLAKE TRAWL OR DOUBLE BEAM-TRAWL. IN USE (OR IN POSITION FOR DRAGGING ON THE BOTTOM).

The Blake trawl (fig. 2) was the joint invention of Mr. Alexander Agassiz, Commander Sigsbee, U.S.N., and Lieut. Ackly, U.S.N., and was used with great success on the several dredging-cruises of the steamer Blake from 1878 to 1880, undergoing, during this time, a few slight improvements to perfect its working. In 1880 it was adopted by the fish-commission for deep-water work, in connection with the old pattern; and in 1883 it was also copied by the French exploring-steamer Talisman. The following description is made up from one of the trawls belonging to the latter party, and differing but slightly from that of the Blake.

The runner-frames, made of bar-iron half an inch thick by three inches wide, form a very broad D-shaped figure, being equally curved above and below in front, and extending thence straight back to the upright hinder end, beyond which the runners project slightly, the overlapping portions being perforated for the attachment of the net. These frames measure three feet and a half in height by four feet in length, and are rigidly connected by two beams of iron gas-pipe, ten feet and three-quarters long and two inches and a quarter in diameter, which screw into brass collars riveted to the inner sides of the runners, — one in front, and one behind. The net, like the frame, is perfectly symmetrical in shape, and consists of a cylindrical or slightly conical bag of stout twine webbing, open at the lower end. Its length may vary from eighteen to twenty-five feet; and, to give it increased strength, a double thickness of webbing is generally employed. The folds formed in tying the lower end of the net for use serve to retain a certain quantity of the fine bottom-material.

The method of attaching the net to the runner-frame is simple. A two-and-a-quarter-inch rope runs around the entire mouth, and is laced to the hinder ends of the runners, and secured to the four hinder corners of the same. In common with the mouth of the net, this rope is left sufficiently slack between the runners on both sides; so that, whichever side is uppermost, the slack of that side curves down to the level of the beams, and does not obstruct the lower half of the opening: the lower slack line naturally curves backward upon the ground. These slack portions of the line are weighted to serve as lead lines.

There is an inner pocket, or trap, to the net, and a series of four glass or cork floats to assist in keeping it expanded. The runner-openings, and the space between the beams, are also closed in with netting. The bridle for the attachment of the drag-rope may be fastened to the fronts of the runners, or carried back to the hinder end of the net, as before explained. Other methods of arranging the net have been tried, but that above described has proved most satisfactory.

Trawl-wings.

It has long been observed, that enormous quantities of small and delicate free-swimming animals, especially Crustacea of the lower orders, come up completely crushed in the mass of material which frequently fills the trawl; and it was also evident that still larger quantities must escape through the coarse meshes of the net. To collect and preserve these forms, Capt. H. C. Chester arranged in 1880 for the use of the fish-commission, in connection with the beam-trawls, a large towing-net, having a rectangular mouth-frame of iron three feet long by eight inches wide, and a moderately fine mesh bag of netting about three feet in length. Into the lower end of this bag is fitted one of the ordinary silk or linen towing-nets for the purpose of retaining the very smallest objects. Two of these towing-nets are fastened to each trawl of either pattern nearly every time they are used; being attached, one at each end of the beam (as shown in fig. 3), by means of a piece of small

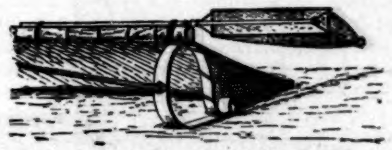

FIG. 3.—THE TRAWL-WINGS ATTACHED TO THE BEAM-TRAWL, IN USE.

gas-pipe lashed by one end to the beam, or extending a short distance into it, if the latter is also of iron. The trawl-wings, as these nets have been christened, give such excellent results, that their appearance at the surface, after a haul, is as anxiously watched for as is that of the trawl proper.

Tangles.

While the use of hempen tangle-swabs attached to the dredge was introduced by the English exploring-steamer Porcupine in 1868 or 1869, the idea that they were worthy of being used separately appears to have originated with Professor Verrill of the fish-commission in 1871; since which time other explorers, both European and American, have employed them to a slight extent in the same way. It has been the experience of this commission, that the combination of tangles with the dredge or trawl is, to say the least, cumbersome; and, following in the wake of either, they generally pick up only the more or less mutilated specimens which have been injured by the iron scrapers or the lead line. By attaching them at the sides, however, as is sometimes done, the latter objection is removed.

The true province of the tangles is a very rocky bottom, where neither the dredge nor trawl can be safely used; and here they perform a real service, notwithstanding the impossibility of extricating the delicate specimens

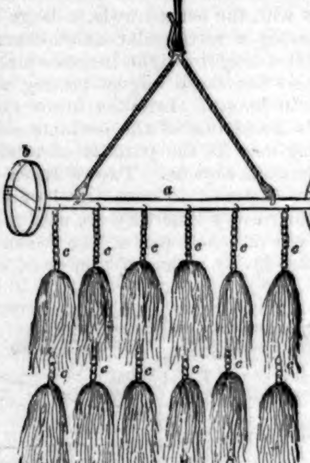

Fig. 4.—The tangles (Verrill's pattern).

from the hempen swabs without injury. They may also be employed on moderately rough bottoms to supplement the work of the dredge; but, used separately, both have, by actual experience, been proved to obtain far better results. On smooth bottoms, it does not seem rational to suppose that the tangles can in any way add to the results afforded by the beam-trawls, properly managed; and several trials, made on rich ground of this character, have shown such to be the case.

A short distance beyond the coast-line, we generally come upon such uniformly smooth bottom, that the beam-trawl can be trusted nearly everywhere. Working in such a region as this, enormous hauls would be obtained day after day; the trawl delivering its specimens in exceptionally good condition, and affording the full variety of life which existed there. During the earlier part of the explorations, alcohol was used at the rate of two to three barrels a day, and certainly better results could not be asked for. At intervals the tangles would be lowered, but they never furnished any thing new; and the pitiable condition of the specimens they brought up, when compared with those from the trawl, caused their use to be discontinued. And what more could be expected of them, when attached to the runners or net of the trawl?

The tangles devised by Professor Verrill, in 1871, were secured to a triangular iron frame, similar to that of the rake-dredge. In 1873, however, they were altered and improved as represented in fig. 4.

They consist of an iron bar, rigidly attached to two rings or wheels, as a framework, from which extend several small iron chains, each carrying from three to five hempen swabs of medium size. The wheels are not intended to revolve, but merely to keep the bar above the ground, so as to prevent its coming in contact with the specimens; and whatever injury befalls

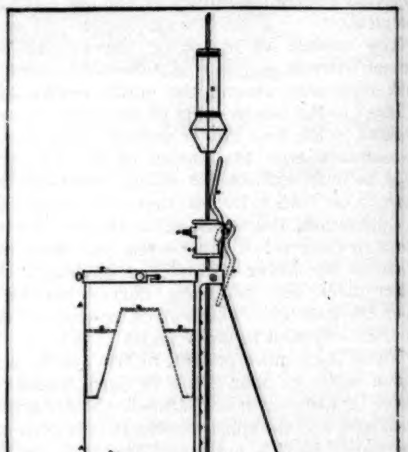

Fig. 5.—Sigsbee's gravitating trap.

the latter must result from their entanglement among the hempen fibres.

Towing-nets.

As to the towing-nets for collecting at the surface, and at depths intermediate between the surface and the bottom, we have but a single noteworthy improvement to mention, — the gravitating-trap of Commander Sigsbee, which was successfully worked on the last dredging-cruise of the steamer Blake. It is designed to traverse rapidly any given vertical space at any required depth, for the purpose of determining the character and abundance of life at different levels. It does not, however, afford the means of obtaining continuous horizontal towings at intermediate depths, unmixed with the life of higher levels; such a result being still a subject for future investigation.

The gravitating-trap (fig. 5) consists of a brass cylinder, two feet long by forty inches in diameter, riveted to a wrought-iron frame, covered with gauze at the upper end, and having a flap-valve opening inward at the lower. It is suspended to the wire dredge-rope on which it travels, by means of a friction-clamp; while at the point below, to which it is to descend, there is a friction-buffer. The weight of the cylinder and its frame, from the manner in which they are suspended, keeps the valve closed until the apparatus has been lowered to the highest level from which it is desired to take the specimen. Every thing being in readiness, a small weight or messenger is sent down the rope, which, on striking the friction-clamp, disengages it, allowing the cylinder-clamp and messenger to descend by their own weight to the buffer. As the cylinder strikes the buffer, the valve closes, and is held in this position, during the hauling-back, by the weight above it. This implement may be worked at any depth, and the distance traversed by the cylinder may be regulated at will. The many details of construction have been purposely omitted.

For the ordinary towing-nets for surface-collecting, and for use in connection with the trawl-wings, silk bolting-cloth, which can be obtained of any size of mesh, has been substituted for the various other kinds of cloth formerly employed. Bolting-cloth, though moderately expensive, is very strong and durable, and the nets constructed of it have given great satisfaction. The towing-net frames are made of heavy brass wire, and are generally circular in shape, though an elongated rectangular frame is sometimes employed.

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE PHILADELPHIA MEETING.

We have made arrangements for publishing reports and abstracts of so many of the papers presented at Philadelphia, that our readers can soon judge for themselves of the scientific importance of the meeting; and we shall therefore restrict our editorial comments, this week, to some general impressions which were formed during the progress of the session.

The intense heat of the first five days was a serious drawback from the pleasure of attendance, but it was the only drawback. It doubtless deterred from the journey a few who would otherwise have been present; but the arrangements of the Philadelphians were so complete, that those who were in the city encountered the minimum of discomfort, and enjoyed the utmost benefits which a great convention can afford. It was particularly fortunate that Saturday was kept free from all sessions, for many persons were thus enabled to devote two days to refreshment by the seashore or in the mountains in the company of their associates and friends. Every thing which could be done by an enlightened and wealthy community, devoted to hospitality, was done to show an interest in, and respect for, the workers in science, American and foreign. Nothing was forgotten or neglected. The permanent officers of the association did their part with the most satisfactory efficiency. Museums, libraries, and collections were freely opened; and the electrical exhibition, though not complete, was far enough advanced to be an attractive and instructive show. The convention of the mining engineers, and the convention of Agassiz clubs, augmented the number of attendants upon the meetings.

The public interest in the sessions, as usual, reached its height at the delivery of the presidential address. On this occasion, Professor Young, as our readers have already discovered, presented a masterly review of the present condition of astronomical science and of the problems which next invite attack. With many bright flashes, his discourse was as orderly as the solar system; and he balanced this view and that with the skill of a trained physicist. It is rare on such anniversaries for a speaker to be so felicitions in the choice and treatment of his theme. We trust that our readers will pardon us for saying, that by the kindness of the lecturer we were able, at the close of his discourse, to distribute the number of *Science* in which it was printed.

We are inclined to think that the custom which puts the president's address in the evening is unwise. It is usually an elaborate essay, depending for its interest more on its matter than on its style; though, in this, style and matter were both excellent. Sometimes, as at the present session, very close attention must be given by ordinary listeners if they would seize the points of the discourse. Why should this lecture be given in the evening, when everybody is tired, when the gas augments the solar heat, and when many are impatient for the social entertainment which is to follow? Why should it not be delivered at a morning session?

So far as the daily newspapers came under our eye, there seems to be a great falling-off in their abstracts of the papers. The reporters seem to be in despair as to what to select from the superabundance of material, and in many cases their choice is hap-hazard. Indeed, it is very difficult for any one to determine from the programme what will be of most interest, or exactly when particular papers will be read. Some 'sifting' or 'grinding' committee seems indispensable to eliminate such papers as are for any reason inappropriate to these gatherings. There should be a survival of the fittest, and the rest should disappear.

We trust the day will come when it will be considered the mark of a bad education to read or speak indistinctly in public, — when bad utterance will be as great an offence against the usages of good society as bad grammar or bad spelling. More than one speaker in Philadelphia has thwarted his own purposes by his low, inarticulate, or suppressed vocalization. Instead of awaking an interest, he has smothered it. Why should college professors speak so poorly as many of them do?

So far as our observations go, the most useful meetings of the sections appear to be those in which a discussion is provoked upon some interesting question, not necessarily on a new point. For example, such debate as took place in the mechanical section, on instruction in mechanics; or that in the physical section, on thunderstorms; or as that proposed in the chemical section, on the best methods of teaching chemistry, — are valued by all who are present, more, even, than elaborate papers which can hardly be appreciated until they are printed.

The 'special committees' of the association did not appear in a very efficient aspect, when the long list of them (eleven in number) was called Monday morning, with but one written and two oral responses. We may also add, that better modes of promoting the work of the association can be devised than these 'general sessions,' which consume the best hour of the morning, and really accomplish very little good.

The number of members enrolled as present, up to Tuesday morning, was 1,157; and many more have since arrived. The members of the British association have been received with great cordiality; and every proposal to continue the friendly relations which have been fostered this summer, and all proposals looking toward an international scientific congress, are received with great favor.

As a whole, we are sure that the Philadelphia meeting is one of the best, if not the very best, which has ever been held.

COLLEGE MATHEMATICS.[1]

PROFESSOR EDDY announced as the subject of his address, the present state of mathematical training in our colleges; its aims, its needs, and its relations to education and to scientific research. It is an article of faith firmly held and oft expressed by the undergraduate, that higher mathematics is a study which can be thoroughly mastered only by exceptional geniuses. One very bad feature in this state of things is, that this sentiment respecting mathematical study is not confined to undergraduates, but is largely shared, not only by the faculties in general, but by the instructors and professors of mathematics as well.

There are various reasons which have led mathematical teachers to this opinion, besides the ill success that has attended their efforts with their pupils. It must be admitted that, too often, the instructors themselves have not become engrossed in their studies, perhaps not even interested in them. That we have in this country no large body of men whose life-work has been, day by day, directed in the line of mathematical investigation, is evident to all. The paucity of important mathematical investigations emanating from this side of the Atlantic is proof of it. But even where the professorial chair is filled by an eager and brilliant mathematician, he often feels the hopelessness of initiating his pupils into this all-absorbing realm of thought in the few brief months at his disposal. Thus it has come to pass, that the study has been used simply as a form of mental discipline or intellectual gymnastics: the object sought

[1] Abstract of an address to the section of mathematics and astronomy of the American association for the advancement of science, at Philadelphia, Sept. 4, by Prof. H. T. Eddy of the University of Cincinnati, vice-president of the section.

was not to learn how to use this the most splendid instrument of intellectual research yet devised by the wit of man.

There is an underlying consciousness running through the whole scheme of education based upon classical study, that the objects of such study are not in themselves of vital importance to the student, but that their value is chiefly to be found in the reflex influence upon the person submitting to its discipline. Pretend to deceive ourselves as we may upon this point, the undergraduate feels this with every breath of his young life. Professor Eddy did not take the position that classical study is in itself a delusion, nor that the ancient languages and philological science are not most worthy and inspiring objects of study for those who really intend to know something of them, or for those whose tastes and capacities fit them for their pursuit; but that this demoniacal spirit of study for the sake of discipline, which possesses our colleges, must be cast out before they can rightly train classical scholars, or stand where they should stand in the forefront of higher culture in the liberal arts: and this by the introduction of a spirit of study very different from the disciplinary spirit, — a spirit which, for the lack of a better name, we may call the scientific spirit; a spirit of sincere and earnest inquiry after knowledge.

There is apparently no reason why the spirit which so largely animates scientific study should be confined to that kind of study, for it is not the nature of the study which determines the spirit in which it shall be pursued. Mathematics is a case very much in point in this regard. The truth is, young men of spirit will not shirk hard work, if they are convinced that by it they can open up any fair field of knowledge which appears desirable. And the speaker said, that, under such influences, he had seen students gain, during the first half of their college course, such familiarity with those branches of higher analysis which are the common groundwork of modern investigation in analytical mechanics and mathematical physics, as to have really open to them the literature of these subjects; and this not in isolated instances merely, but with class after class. It is popularly supposed, as before stated, that the number fitted by nature for mathematical study is small. Such, Professor Eddy has been convinced against his preconceived opinions, is not the fact. It is a study as much sought after, and pursued as eagerly, as any other branch of liberal study; provided only that the teachers thereof are themselves men who have a live interest in the subject, are capable, patient, and apt at giving instruction.

Professor Eddy then discussed somewhat more in detail the scope of mathematical instruction in college. The geometry of Euclid, which should be relegated to the schools, has long held a part of honor in the mathematics of the college course. The cause for this is easily seen. It is a subject which lends itself, more readily than any other branch of mathematics, to the form of discipline in vogue. It certainly is a matter of vastly more importance as a piece of mathematical training, to have the student of Euclid acquire the habit of discovering for himself the demonstration of new propositions, than that the study of Euclid should be made a huge memoriter exercise, as is usually done in college. The clear apprehension of geometrical relations, aside from the language describing them, is of the first importance, and may be cultivated by any work which deals with such relations.

Several other mathematical subjects could well be covered before entering college. These are the elementary parts of algebra, the numerical solution of plane triangles, the practical use of logarithmic tables, and the elementary ideas of analytical geometry. The field would then be cleared, so that the training in all those forms of analysis which are distinctively modern, and which must needs be taught by men in sympathy with its methods, would fall within the years of the college course.

Objection may be made to the amount of mathematical preparation which it is here proposed to put into the schools.

But what ought the actual scope of mathematical instruction to be during the college course?

It seems superfluous to say, that, without the mastery of the infinitesimal calculus, any mathematical culture of importance is hopeless; and that a knowledge of its methods, accompanied by facility in their employment, is absolutely essential to the understanding of the exact sciences.

Calculus is not omitted from the scheme of study of any classical college in this country; but it is hardly too much to say, that, so far as any real knowledge of it is concerned, it might as well be omitted from them all.

The text-books in use are of such very elementary and defective character, that no sufficient knowledge of the subject can be obtained from them. They are constructed on the plan of omitting almost every thing which may present any special difficulty. It has been in effect assumed by those imbued with the disciplinary spirit, that a knowledge of this subject could be conveyed to the student by daily recitation upon its principles and developments. This is as useless an attempt as to try to prepare an army for the battle-field by a daily lecture instead of a daily drill, or by explaining tactics instead of practising them. The important processes actually employed in calculus are not so very numerous, nor are they especially difficult to acquire. No real use, however, can be made of its methods until these are acquired. It must often happen that the full significance of such processes is not apprehended until long after they are employed with dexterity. Certain it is that such dexterity and familiarity conduce wonderfully to their correct comprehension.

The daily marking system is perhaps the most characteristic and most pernicious expression of the college disciplinary spirit. How have the evils of that system been intensified in our larger colleges by the wholesale manner in which the work is done! The work of recitation and instruction can, no doubt, often be advantageously combined; but what is the probability that valuable instruction will be communicated during the hour to which the exercise is confined, when the number of students in the recitation-room is thirty, forty, or even fifty? What a perversion of the purposes of the noble endowments for higher education, to expend almost the entire energy of the teaching force of the many institutions which adopt this system, in a daily effort to weigh with minutest accuracy the fidelity with which assigned tasks have been committed to *memory!* The most diverse views may be entertained as to whether the college course can embrace analytical mechanics, or the theory of determinants (now so universally used), or whether it can omit vector and quaternion analysis. When, however, it is known that in a small western college graduating less than a dozen annually, we have now had for years volunteer classes, pursuing all these and other subjects annually, with success, the possibility of including them in a college curriculum must be acknowledged.

In conclusion, Professor Eddy wished to call for reform in our mathematical teaching. Let it not be so conducted that he who has neither taste for the study, nor special knowledge of it, stands on an equal footing as a teacher with the man of real mathematical insight. Now is a favorable time for revising our estimates of what can and ought to be done in this field. Higher mathematical culture has commenced a new and fruitful growth in this country in various places; and an association of the mathematicians of this country might be of service for the purpose of concerted action in improving the mathematical training in our colleges.

WHAT IS ELECTRICITY?[1]

ALL Professor Trowbridge hoped to do was to make his audience ask themselves the question with more humility and a greater consciousness of ignorance. We shall probably never know what electricity is, any more than we shall know what energy is. What we shall be able, probably, to discover, is the relationship between electricity, magnetism, light, heat, gravitation, and the attracting force which manifests itself in chemical changes. Fifty years ago scientific men attached a force to every phenomenon of nature: thus there were the forces of electricity and magnetism, the vital forces, and the chemical forces. Now we have become so far unitarian in our scientific views, that we accept treatises on mechanics which have the one word 'Dynamik' for a title; and we look for a treatise on physics which shall be entitled 'Mechanical philosophy,' in which all the phenomena of radiant energy, together with the phenomena of energy which we entitle electricity and magnetism, shall be discussed from the point of view of mechanics. What we are to have in the future is a treatise which will show the mechanical relation of gravitation, of so-called chemical attracting force, and electrical attracting force, and the manifestations of what we call radiant energy. We have reduced our knowledge of electricity and magnetism to what may be called a mechanical system, so that in a large number of cases we can calculate beforehand what will take place, and we are under no necessity of trying actual experiments. It is probable, for instance, that the correct form of a dynamo-machine for providing the electric light can be calculated and the plans drawn with as much certainty as the diagrams of a steam-engine are constructed. We may congratulate ourselves, therefore, in having a large amount of systematic knowledge in electricity: and we see clearly how to increase this systematic knowledge; for we have discovered that a man cannot expect to master the subject of electricity who has not made himself familiar with thermo-dynamics, with analytical mechanics, and with all the topics now embraced under the comprehensive title of 'physics.'

Out of all the theories of electricity, the two-fluid theories, the one-fluid or Franklin theory, and the various molecular theories, not one remains to-day under the guidance of which we are ready to march onward. We have discovered that we cannot speak of the velocity of electricity. All that we can truly say is, we have a healthy distrust of our theories, and an abiding faith in the doctrine of the conservation of energy.

It is one thing to become familiar with all the applications of the mechanical theory of electricity, and another to make an advance in the subject so that we can see the relations of electrical and magnetic attraction to the attraction of gravitation and to what we call chemical attraction. To this possible relationship, Professor Trowbridge wished to call attention. The new advances in our knowledge of electrical manifestations are to come from the true conception of the universality of electrical manifestations, and from the advance in the study of molecular physics. When we let an acid fall from the surface of a metal, the metal takes one state of electrification and the drop of acid the other: in other words, we produce a difference of electrical potential. On the other hand, a difference of electrical potential modifies the aggregation of molecules. The experiments of Lippman are well known. He has constructed an electrometer and even a dynamo-electric machine which depend upon the principle that the superficial energy of a surface of mercury covered with acidulated water is modified when a difference of electrical potential is produced at the limiting surfaces. The manifestations of what is called superficial energy, — that is, the energy manifested at the surface of separation of any two substances, — and the effect of electricity upon the superficial energy, afford much food for thought. There have always been two parties in electricity, — one which maintains that electricity is due to the contact of dissimilar substances, and the other party which believes that the source of electrical action must be sought in chemical action. Thus, according to one party, the action of an ordinary voltaic cell is due to the contact, for instance, of zinc with copper; the acid or solution of the cell merely acting as the connecting

[1] Abstract of an address before the section of physics of the American association for the advancement of science, at Philadelphia, Sept. 4, by Prof. JOHN TROWBRIDGE of Harvard college, Cambridge, vice-president of the section.

link between the two. According to the other party, it is to the difference of the chemical action of the metals on the connecting liquid, that we must attribute the rise and continuance of the electrical current. The electromotive force of a voltaic cell is undoubtedly due to the intrinsic superficial manifestation of energy when two dissimilar metals are placed in connection with each other either directly or through the medium of a conducting liquid. The chemical action of the liquid brings new surfaces of the metals constantly in contact; moreover, we have the difference of superficial energy between the liquid and the metals, so that our expression for electromotive force is far from being a simple one: it contains the sum of several modifications of superficial energy at the surfaces of the two metals and at the two boundaries of the liquid and the metals.

We have again a development of electromotive force by the mere contact of the metals at different temperatures. The electrical current that arises is due to the difference of superficial energy manifested at the surface of the two junctions. We know that the action is on the surface, for the size of the junctions does not affect the electromotive force. Suppose that we should make the metals so thin that an ultimate molecule of iron should rest against an ultimate molecule of copper, should we not arrive at a limit, at a definite temperature of the conversion of molecular vibration into electrical energy? And also, when our theory is perfected of the number of molecules along a linear line of copper against a linear line of zinc which can produce a current of electricity of a given strength, — the jostling, so to speak, of these ultimate molecules of two metals at different temperatures might form a scientific unit of electromotive force in the future science of physical chemistry. By means of an alloy we can apparently modify the superficial energy at the surface of a solid. Thus an alloy with a parent metal will give a varying electromotive force. If we could be sure that an alloy was always of a definite chemical composition, and not a more or less mechanical admixture, it seems as if we could get closer to the seat of electromotive force by a number of quantitative measurements. Unfortunately, the physical nature of alloys is not definitely known, and there is little coherence or regularity in our measurements of their electromotive force. We can modify the superficial energy of metals, not only by melting metals together, but also by grinding them to a very fine powder, and compressing them again by powerful means into solids more or less elastic, and then examining their superficial energy which is manifested as electromotive force. Professor Trowbridge is still engaged upon researches of this nature; and, if the work is not brilliant, he hopes that it will result in the accumulation of data for future generalization.

The subject of thermo-electricity has been eclipsed by the magnificent development of the dynamo-electric machines; but we may return to thermo-electricity as a practical source of electricity. Professor Trowbridge has been lately occupied in endeavoring to modify the difference of potential of thermo-electric junctions by raising one junction to a very high temperature under great pressure; for it is well known that the melting-point of metals is raised by great pressure. If the metal still remains in the solid state under great temperature and great pressure, can we not greatly increase the electromotive force which results from the difference of superficial energy manifested at the two junctions?

It is evident that our knowledge of electricity will increase with our knowledge of molecular action, and our knowledge of molecular action with that which we call attractive force. It is somewhat strange, that, although we are so curious in regard to electricity, we seldom reflect that gravitation is as great a mystery as electrical attraction. What is the relation between electricity and magnetism and gravitation and what we call the chemical force of attraction?

The question of the connection between electricity and gravitation dwelt much in Faraday's thoughts. He failed, however, to find the slightest relation between gravitation and electricity; and he closes his account of his experiments with these words: "Here end my experiments on this subject for the present, but I feel the conviction that there must be some connection between electricity and gravitation." Was the direction in which he experimented the true direction to look for a possible relation? and cannot the refined instruments and methods of the electrical science of the present aid us in more promising lines of research? If we could prove that whenever we disturb the relative position of bodies, or break up the state of aggregation of particles, we create difference of electrical potential; and, moreover, if we could discover that the work that this electrical potential can perform, together with the heat that it developed by the process, is the complete work that is done on the system against attractive force, or as so-called chemical attractive force, — we should greatly extend our vision of the relation of natural phenomena. And thus pursuing the line of argument of his address, Professor Trowbridge ventured to state an hypothetical law which it seemed to him is at least plausible: That "whenever the force of attraction between masses or molecules is modified in any way, a difference of electrical potential results." Is it not reasonable to suppose that certain anomalies which we now find in the determinations of specific heats of complicated aggregation of molecules are due to our failure to estimate the electrical equivalent of the movements and interchanges of the molecules? Let us take the case of friction between two pieces of wood: is it not possible that the friction is the electrical attraction which results from the endeavor to connect the phenomenon of superficial energy with electrical manifestations, that the friction between two surfaces is modified by keeping these surfaces at a difference of electrical potential? In Edison's motophone, we see this exemplified in a very striking manner.

Professor Trowbridge's own studies have been chiefly in the direction of thermo-electricity and in the subject of the electrical aspect of what we call superficial energy. These experiments so far deepen the belief that any change in the state of aggregation of particles, — in other words, any change which results in a modification of attracting force, — whether gravitative or the commonly called chemical attracting forces, results in an electrical potential; and conversely, that the passage of electricity through any medium produces a change of aggregation of the molecules and atoms. If we suppose that radiant energy is electro-magnetic, cannot we suppose that it is absorbed more readily by some bodies than by others, or, in other words, that its energy is transferred, so that with the proper sense we would perceive what might be called electrical color, or, in other words, have an evidence of transformations of radiant energy other than that which appeals to us as light and color? We have arrived at the point in our study of electricity where our instruments are too coarse to enable us to extend our investigations. Is not the physicist of the future to have instruments delicate enough to measure the heat equivalent of the red and the yellow and the blue violet rays of energy? instruments delicate enough to discover beats of light as we now discover those of sound? The photographer of to-day speaks in common language of handicapping molecules by mixing gums with his bromide of silver, in order that their rate of vibration may be affected by the long waves of energy. Shall we not have the means of obtaining the mechanical equivalent of such handicapped vibrations? We have advanced; but we have not answered the question which filled the mind of Franklin, and which fills men's minds to-day: What is electricity?

CHEMICAL AFFINITY.[1]

PROFESSOR LANGLEY first reviewed the history of chemical theory, and called attention to the final extinction of the term 'affinity' in the chemical literature of the present day.

Shortly after the opening years of the present century, three general methods were indicated for the study of the force of affinity. Instead of being successively taken up and abandoned, like all preceding speculations, they have remained steadily in use during the eighty years which have intervened, and to-day they are still the most promising means at our disposal. These three methods may be called the thermal, the electrical, and the method of time or speed. It will be convenient to consider each one separately.

The most important generalization to be drawn from thermo-chemical phenomena is, that the work of chemical combination, or the total energy involved in any reaction, is very largely influenced by the surrounding conditions of temperature, pressure, and volume; and the conclusion they force upon us in regard to the nature of affinity is most important, namely, that this force in accomplishing work is dependent, like all other forces, on the conditions exterior to the reacting system which limit the possible amount of change. Affinity is therefore at last definitely removed from the category of those mystical agents, so often imagined by our predecessors in a less critical age, which had no correlation with the general forces of nature.

Under the title 'dissociation,' St. Claire Deville gave to the chemical world, in 1857, a new and fruitful method of investigating the nature of compounds by determining the temperature at which bodies break up or are dissociated. The laws developed by Deville and his successors in this field show us, that, after the point is reached at which decomposition commences, the further breaking up is determined by the pressure of the evolved products of the reaction, so that the permanence of the body depends on the magnitude of two variables, pressure and temperature, either of which may be varied at will through a wide range.

The electrical method of dissecting chemical forces has been followed less actively than the thermal one. Besides the well-known experimental contributions of Davy, Becquerel, and Faraday, may be mentioned Joule's researches on the heat absorbed during electrolysis, and especially the work of C. R. Adler Wright, on the 'determination of affinity as electromotive force.' The general outcome of these researches is, that the products of electrolysis are so numerous, and so varied by the results of secondary actions, that it is very doubtful whether the electromotive force measured is that due solely to the union of those atoms which are indicated by the principal equation of the reaction.

The method of time or speed of chemical reactions has a history as old as that of its two associates; but the story is much less eventful, for very little work has been done in this field. The most notable work has been done by Gladstone and Tribe, by ascertaining the rate at which a metallic plate could precipitate another metal from a solution.

To these general methods for studying the problems of chemical dynamics, should be added the investigation of the action of mass, by Gladstone, in his well-known color work on the sulphocyanide of iron; of the chemical action of light, by the late J. W. Draper in this country, and Prof. H. E. Roscoe in England, as well as Becquerel in France, — pioneers who have since been followed by a host of students of scientific photography.

In the review just given, no attempt has been made to do more than glance at the important contributions to the theory and methods of measuring affinity. Many names have been passed by, and much work has been necessarily ignored.

The history of the various modifications and additions which have been made to the primitive conception of the nature of affinity, when briefly summarized, appears to be this: Hippocrates held that union is caused by a kinship, either secret or apparent, between different substances. Boerhaave believed affinity to be a *force* which unites unlike substances. Bergman and Geoffroy taught that union is caused by a selective attraction; and therefore they called it 'elective affinity.' Wenzel and his success-

[1] Abstract of an address to the section of chemistry of the American association for the advancement of science, at Philadelphia, Sept. 4, by Prof. J. W. LANGLEY, of the University of Michigan, Ann Arbor, Mich., vice-president of the section.

ors showed that affinity is definite in action and amount: it has limits, or proceeds *per saltum*. Berthollet contended that affinity is not definite: he proves that it is often controlled by the nature and the masses of the reacting bodies. Dalton, Berzelius, Wollaston, and others held, on the contrary, this force to be definite, and to act *per saltum*: it is a power which emanates from the atom. Davy, Ampère, and Berzelius believed affinity to be a consequence of electrical action. Avogadro in one way, and Brodie in another, show us affinity exerted by molecules as well as atoms. It is a force which binds together, not only particles of the same substance, but also of heterogeneous substances. From the fact of the actual existence of radicles, and from the phenomena of substitution, was developed the notion of position, and that, therefore, affinity varied with the structure of the body as well as with its composition. The differences between the number of atoms which are equal to hydrogen in replacing power have led to the doctrine of valence, which, if it has any influence on theories of affinity, shows that this property of matter has two distinct concepts, — one, its power of attracting a number of atoms; the other, its power of doing work or evolving energy. These two attributes seem to be in no way related to each other. Mendelejeff and Lothan Meyer have shown, by the facts which are grouped under the title 'periodic law,' that the properties of elements seem to be repeating functions of the atomic weight. Hence affinity is connected in some way with that same property, which is also shown by the differential action of gravitation on the absolute chemical unit of matter. Finally, Williamson, Kekulé, and Michaelis have suggested that combination is brought about and maintained by incessant atomic interchange; hence, that affinity is fundamentally due to some form of vibration.

The idea which seemed so simple and natural a one to Hippocrates has grown successively more complex and less sharply defined; and we are compelled to admit that the years have not brought the theory of affinity to a state of active growth. Chemists have more and more turned their attention to details, to accumulating methods of analysis and synthesis, to questions of the constitution of salts, to discussions about graphic and structural formulae, and to hypotheses about the number and arrangement of atoms in a molecule; but they have not, until quite recently, made systematic attempts to measure the energies involved in reactions. Why? The answer can be found mainly in two reasons. First, the word 'affinity' is in bad odor. We see how enormously complicated the phenomena of chemical action have become, and we have lost all faith in hypotheses which can be evolved by the mere force of metaphysical introspection. Second, there is a more important reason, arising from what has hitherto been the traditional scope of our science. Chemistry alone of the physical sciences has offered no foothold to mathematics; and yet all her transformations are governed by the numbers which we call 'atomic weights.' What is it which causes chemistry, so pre-eminently the analytic science of material things, to be the only one which does not invite the aid of mathematics? It is because three fundamental conceptions underlie physics, while only two serve the needs of the chemist. If a term so much used just now by transcendental geometers may be borrowed, one would say that physics is a science of three dimensions, while chemistry is a science of two dimensions. In the first, nearly every transformation is followed by its equation of energy; and this involves the concepts space, mass, time: while, in the second, an ordinary chemical equation gives us the changes of matter in terms of space and mass only; that is to say, in units of atomic weight and atomic volume.

Think for a moment what physics would be to-day without those grand generalizations, Newton's theory of gravitation, Young's undulatory theory of light, the dynamic theory of heat, the kinetic theory of gases, the conservation of energy, and Ohm's law in electricity. Every one of these, except the last, is a dynamic hypothesis, and involves velocity — that is, time — as one of its essential parts. In comparison with the above, all ordinary chemical work may be termed the registration of successive static states of matter. The analyst pulls to pieces, the synthetic chemist builds up; each records his work as so many atoms transferred from one condition to another, and he is satisfied to exhibit the body produced quietly resting in the bottom of a beaker, motionless, static. The electrolytic cell tells us the stress of chemism for specified conditions as electromotive force; the splendid work done in thermo-chemistry enables us to know the whole energy involved when A unites with B, or when A B goes through any transformation however intricate, but it does not inform us of the dynamic equation which accompanies them, and which should account for the interval between the static states.

Whenever we look outside of chemistry, we find that the lines of the great theories along which progress is making are those of dynamic hypotheses. If we go to our biological brethren, we see them too moving with the current; the geologist studies upheavals, denudation, rate of subsidence, glacial action, and all kinds of changes, in reference to their velocity; the physiologist is actively registering the time element in vital phenomena, through the rate of nervous transmission, the rate of muscular contraction, the duration of optical and auditory impressions, etc.; even the sociologist is beginning to hint at velocities, as, indeed, we should expect in a student of revolutions; and we cannot ignore the fact that all the great living theories of the present contain the time element as an essential part. The speaker could but think the reason that chemistry has evolved no great dynamical theory, that the word 'affinity' has disappeared from our books, and that we go on accumulating facts in all directions but one, and fail to draw any large generalization which shall include them all, is just because we have made so little use of the fundamental concept, time. To expect to draw a theory of chemical phenomena from the study of electrical decompositions and of thermo-chemical data, or from even millions of the customary static chemical-equations, would be like hoping to learn the nature of gravitation by laboriously weighing every moving object on the earth's surface, and recording the foot-pounds of energy given out when it fell. The simplest quantitative measure of gravity is, as every one knows, to determine it as the acceleration of a velocity: when we know the value of g, we are forever relieved, in the problem of falling bodies, from the necessity of weighing heterogeneous objects at the earth's surface, for they will all experience the same acceleration. May there not be something like this grand simplification to be discovered for chemical changes also?

The study of the speed of reaction has but just begun. It is a line of work surrounded with unusual difficulties, but it contains a rich store of promise. All other means for measuring the energies of chemism seem to have been tried except this: is it not, therefore, an encouraging fact, that to the chemists of the nineteenth century is left for exploration the great fruitful field of the true dynamics of the atom, the discovery of a time rate for the attractions due to affinity?

THE MISSION OF SCIENCE.[1]

After thanking the section for the honor conferred upon him by electing him their chairman, and referring to the success of the meeting of the British association at Montreal, Professor Thurston announced as the subject of his address, 'The mission of science.' He spoke of his address, as vice-president at St. Louis in 1878, on the philosophic method of the advancement of science, in which he had called attention to the need of specialists, amply supplied with the proper means, to do the work of observing, collecting, and co-ordinating the results of observation. As an all-important factor in this the modern system of scientific investigation, he had spoken of the men who have given, and who are still generously and liberally giving, material assistance by their splendid contributions to the scientific departments of our colleges and of our technical schools.

It may well be asked, What is the use, and what is the object, of systematically gathering knowledge, and of constructing a great, an elaborate system, having the promotion of science as its sole end and aim? What is THE MISSION OF SCIENCE? The great fact that material prosperity is the fruit of science, and that other great truth, that, as mankind is given opportunity for meditation and for culture, the higher attributes of human character are given development, are the best indications of the nature of the real mission of science, and of the correctness of the conclusion that the use and the aim of scientific inquiry are to be sought in the region beyond and above the material world to which those studies are confined.

It being granted that the mission of science is the amelioration of man's condition, it becomes of importance to consider the way in which our knowledge is increased. While the scientific method of advancement of science is evidently that which will yield the greatest returns, it is not the fact that we are indebted to such philosophic methods for the production of the modern sciences. The inventor of gunpowder lived before Lavoisier; the mariner's compass pointed the seaman to the pole before magnetism took form as a science; the steam-engine was invented and set at work, substantially in all essential details as we know it to-day, before a science of thermo-dynamics was dreamed of.

But all this is of the past. Science has attained a development, a stature, and a power, that give her the ability to assume her place in the great scheme of civilization. Hereafter she will direct and will lead. The blind, scheming ways of the older inventor will give place to the exact determination, by scientific methods, of the most direct and most efficient way of reaching a defined end, — methods now daily practised by the engineer in designing his machinery.

It is only in modern times, and since the old spirit of contempt for art, and of reverence for the non-utilitarian element in science, has become nearly extinguished, and since our systems of education have begun to include the study of physical science, that we have had what is properly called a division of 'applied science.' In the days of classical learning, science was only valued as it developed a system of purely intellectual gymnastics. Archimedes was the most perfect prototype, in those days, of the modern physicist and mechanician, of the scientific man and engineer; yet he, and all his contemporaries, esteemed his discovery of the relation between the volumes of the cylinder and the sphere more highly than that of the method of determining the specific gravity of a solid, or the composition of an alloy, and deemed the quadrature of the parabola a greater achievement than the theory of the lever which might 'move the world.' His enumeration of the sands of the seashore was looked upon as a nobler accomplishment than the invention of the catapult, or of the pump, which, twenty-one centuries after his death, still bears his name.

No system of applied science could exist among people who had no conception of the true mission of science; and it was not until many centuries had passed, that mankind reached such a position, in their slow progress toward a real civilization, that it became possible to effect that union of science and the arts which is the distinguishing characteristic of the age in which we live.

In illustration of the gradual evolution and growth of correct theory, and of this slow development of rational views, of the methods of scientific deduction, and of the invariably tardy progress from a beginning distinguished by defective knowledge and inaccurate logic, in the presence of what are later seen to be

[1] Abstract of an address to the section of mechanical science of the American association for the advancement of science at Philadelphia, Sept. 4, by Prof. R. H. Thurston of Stevens institute, Hoboken, N.J., vice-president of the section.

plainly visible facts, and of what ultimately seem obvious principles, observe the rise and progress of our hardly yet completed theory of that greatest of human inventions, the steam-engine.

Studying the history of the development of this theory, it cannot fail to become strikingly evident that, throughout, experimental knowledge and practical construction have been constantly in advance of the theory; and that the science of the conversion of heat-energy into mechanical power has, in all stages of this progress, come in simply to confirm general conclusions, previously reached by deduction from experience and observation, to give the reasons for well-ascertained facts and phenomena, and often — not always promptly or exactly — to define the line of improvement, and the limitations of such advance.

The theory itself began by the correlation of the facts determined by the experiments of Rumford and Davy at the beginning of the century, those announced by Joule and Thomson many years later, and the laws developed by Clausius, Rankine, and Thomson, at the middle of the century. But Watt had discovered, a hundred years ago, the facts which have since been found to set limits to the efficiency of the engine. Smeaton, in many respects the greatest mechanical engineer of his time, made practically useful application of the knowledge so acquired, and endeavored to secure immunity from these wastes by thoroughly philosophical methods. Clarke, a generation ago, showed how the losses first detected by Watt set a definite limit, under the conditions of familiar practice, to the gain to be secured by the expansion of steam; and Cotterill, within a few years, has shown, by beautiful methods of treatment, their magnitude, and how these wastes take place. Hirn and Leloutre, in France, have similarly thrown light upon the phenomena of 'cylinder condensation,' and De Freminville has suggested the method of remedy. Yet it is only now that we are beginning to see that the philosophy of heat-engines is not simply a thermodynamic theory, and that it involves problems in physics, and a study of the methods of conduction and transfer of heat, without doing work from point to point in the engine. We are only now learning how to apply the knowledge gained by Isherwood twenty years ago, and by Hirn and by Clarke still earlier, in solving the problem of maximum efficiency of the steam-engine. We have only now discovered that the 'curve of efficiency,' as Prof. Thurston has called it, is not the curve of mean pressure for 'adiabatic' expansion, as Rankine called it; for 'isentropic' expansion, as Clausius would call it; but that it is a curve of very different form and location, and that it is variable with every physical condition affecting the working of the expanding fluid in the engine. We have only now learned that every heat-engine has a certain 'ratio of expansion for maximum efficiency,' which marks the limit to gain in economy by expansion; which limit is fixed for each engine by the nature of the expansion, and the method and extent of wastes of heat. All the facts of this case were apparently as obvious, as easily detected and weighed in their influence upon the theory of heat-engines, years ago as to-day. Even the latest phase of the current discussion of efficiencies of heat-engines, that relating to their commercial efficiency, would seem to have been as ready for development a generation ago, when first noted by Rankine, as to-day; yet what is now known as a simple and easily formulated theory has been several decades in growing into shape, notwithstanding that all the needed facts were known, or readily determinable, at the very beginning of the period marked by its evolution. It is only within a year or two that it has become possible to say that the theory of the steam-engine, as a case in applied mechanics, has become so complete that the engineer can safely rest upon it in the preparation of his designs, and in his calculations of power, economy, and commercial efficiency.

Professor Thurston then referred to the knowledge now being collected as to the strength, elasticity, and enduring capacity of the materials used in construction. But the slow progress of scientific development in matters relating to common practice in the useful arts is hardly less remarkable than the difficulty with which scientific principles, even when well established and well known among scientific and educated men, sink down into the minds of the masses. Perhaps no principle in the whole range of physical science is better established and more generally recognized than that which asserts the maximum efficiency of fluid in heat-engines to be a function simply of the temperatures of reception and rejection of heat, and to be absolutely independent of the nature of the working-fluid.

This was shown by Carnot sixty years ago, and has been considered one of the fundamental principles of thermo-dynamics from that time to this. Nevertheless, so rarely is it comprehended by mechanics, and so difficult is it for the average mind to accept this truth, that the most magnificent fallacies of the time are based upon assumptions in direct contradiction of it. The various new 'motors' recently brought before the public with the claim of more than possible perfection have taken hundreds of thousands of dollars, within the past two or three years, from the pockets of credulous and greedy victims. It is not sufficient to declare the principle: the comparison of steam with ether, and of air or gas with carbon-disulphide or chloroform, must be made directly, and the results presented in exact figures, before the unfortunate investor, whose rapaciousness is too often such as to cause him to give ear to the swindler rather than to the well-informed and disinterested professional to whom he would ordinarily at once go for advice, can be induced to withdraw from the dangerous but seductive scheme. It is true that the principle does not as exactly apply to a comparison of efficiencies of machine, and that the vender of new motors usually seizes upon this point as his vantage-ground; but a careful comparison of the several fluids, both as to efficiency of fluid and efficiency of machine, throughout the whole range of temperatures and pressures found practicable in application, such as has recently been made under Prof. Thurston's direction, shows that the final deduction is substantially the same for all the usually attainable conditions of practice, and further, that, of all the available fluids, steam is fortunately the best.

That the results of scientific investigation may be the more readily appreciated, it is necessary that the study of physical science should be more thorough in our schools. The stereotyped argument for the retention of the old system of education to the exclusion of the new, was, and is to-day, the assertion that the old system strengthened the intellect and broadened the views of the student, while the new subjects are *merely useful;* but the wisdom and the expediency of a modification of old ways, in this respect, is now rapidly becoming acknowledged, and the new education may be considered as fairly and safely introduced. Science will never, we may be sure, displace entirely the older departments of education; but science will henceforth take a place beside them as no less valuable for mental discipline.

With science recognized as a respectable companion of the dead languages, we shall have better trained students, — students who will be better able to lead in the industries, and so aid material prosperity. As it is the duty of government to so regulate affairs that each man may have the power of improving his condition to the utmost, so will it be the duty of science to point out to government how it may direct its regulations to the greatest advantage of the individual. Men of science, each in his own department, are the natural advisers of the legislator. Citizens and legislators are both entitled to claim this aid from those who have made the sciences of the several arts their special study, and from those who have devoted their lives to the study of the sciences of government, of social economy, and of ethics.

Of all the many fields in which the men of science of our day are working, that which most nearly concerns us, and that which is of most essential importance to the people of our time, is that department of applied science which is most closely related to the industries of the world, — mechanics. The development of new industries becomes as much a part of the work of science in the future as is the improvement of those now existing. The new industries must evidently be mainly skilled industries, and must afford employment to the more intelligent and more finely endowed of those to whom our modern systems of education are offering their best gifts. The enormous advancement of the intellectual side of life must inevitably, it would seem, result in the production of a race of men peculiarly adapted to such environment as science is rapidly producing. Thus accomplishing, under the guidance of science, such tasks as lie before him to accomplish, the 'coming man,' with his greater frontal development, his increased mental and nerve power, his growing endurance and probably lengthening life, will be the greatest of the products of this scientific development, and the noblest of all these wonderful works.

THE CRYSTALLINE ROCKS OF THE NORTH-WEST.[1]

UNTIL very recently, it has been the practice of geologists, almost without exception, to refer every crystalline rock in the north-west either to the Huronian or to the Laurentian. But when, on more careful examination, it is found that this nomenclature is imperfect, we are thrown into much difficulty and doubt. In order that some of the difficulties of the situation may be made clear, Professor Winchell proposed to review concisely the broad stratigraphic distinctions of the crystalline rocks that have lately been studied in Michigan, Wisconsin, and Minnesota.

Omitting the igneous rocks, which in the form of dikes cut through the shales and sandstones of the cupriferous formation, and are interbedded with them in the form of overflows, we may concisely arrange the crystalline rocks, disregarding minor differences and collating only the broad stratigraphic distinctions, in the following manner, in descending order: 1°. Granite and gneiss with gabbro; its thickness is unknown, but certainly reaches several hundred feet. 2°. Mica schist; maximum thickness, five thousand feet. 3°. Carbonaceous and arenaceous black slates, and black mica schists; thickness, twenty-six hundred feet. 4°. Hydro-mica and magnesian schists; maximum thickness, forty-four hundred and fifty feet. 5°. Quartzite and marble; normal thickness, from four hundred to a thousand feet. 6°. Granite and syenite with hornblendic schists; thickness unknown, but very great.

These six great groups compose, so far as can be stated now, the crystalline rocks of the north-west. Their geographic relations to the non-crystalline rocks, if not their stratigraphic, have been so well ascertained that it can be stated confidently that they are all older than the cupriferous series of Lake Superior, and hence do not consist of, nor include, metamorphosed sediments of Silurian, or any other age. The term 'Silurian' here is understood to cover nothing below the base of the Trenton.

Examining these groups more closely, we find: 1°. We have, beneath the red tilted shales and sandstones, a great granite and gabbro group. The gabbro is certainly eruptive, but the associated granite and gneiss are probably metamorphic. The gabbro does not always appear where the granite is present; but in other places these rocks are intricately mingled, although the gabbro can be considered in general as the underlying formation. 2°. Below this granite and gabbro group is a series of strata that may be designated by the general term 'mica schist group.' This division is penetrated by veins and masses of red biotite-granite, which appear to be intrusive in somewhat the same manner as the red granite in the gabbro. These granite veins penetrate only through the overlying gabbro and this underlying mica schist.

[1] Abstract of an address to the section of geology and geography of the American association for the advancement of science at Philadelphia, Sept. 4, by Prof. N. H. WINCHELL of the University of Minnesota, Minneapolis, Minn., vice-president of the section.

They are wanting or comparatively rare throughout the rest of the crystalline rocks. 3°. The next lower grand division might be styled the 'black mica slate group.' This group contains much carbon, causing it to take the form of graphitic schists, in which the carbon sometimes amounts to over forty per cent. These schists are frequently quartzose and also ferruginous. Associated with these black mica slates, which often appear also as dark clay-slates, are actinolitic schists; the whole being, in some places, interstratified with diorite. 4°. Underneath this is a very thick series of obscure hydro-micaceous and greenish magnesian schists, in which, along with gray quartzite and clay slates, occur the most important deposits of hematitic iron-ore. This division of the crystalline rocks has numerous heavy beds of diorite. 5°. Below this series of soft schists is the great quartzite and marble group. The marble lies above the quartzite, and in the Menominee region has a minimum thickness of at least a thousand feet. This is a most persistent and well-marked horizon. In northern Minnesota, the great slate-conglomerate of Ogishke Muncie Lake, with a thickness exceeding six thousand feet, seems to represent the lower portion of the great quartzite of this group, and to be the equivalent of the lower slate-conglomerate of the 'typical Huronian' in Canada.

Now, the difficulties of the situation arise when we cast about to find names for these parts. What are the eastern representatives of these western groups, and by what designations shall they be known?

We meet, at the outset, with the question, Is there a formation such as claimed by Emmons, — the Taconic? On this geologists are yet divided. Having given the subject very careful consideration, Professor Winchell was ready to state his very positive conviction that Dr. Emmons was essentially right, and that the Taconic group will have to be recognized by geologists, and adopted in the literature of American geology.

In the first presentation of the Taconic system, Dr. Emmons extended it geographically too far east, and unfortunately chose a name for it which is appropriate only to a part of that eastward extension. Dr. Emmons's claim, however, in all its essential points, remains intact. This consists in the existence of a series of sedimentary deposits, largely metamorphic, below the Potsdam sandstone, and separating the Potsdam from the crystalline rocks known as 'primary' in an orderly chronological scheme. It is not necessary to refer to the controversies that arose from the creation of the imaginary Quebec group, nor to characterize in deserved terms the attempt to bury the Taconic in the Quebec coffin.

There may be reasons why the current literature of American geology is almost silent respecting the great work of Emmons, and why the Taconic is not known among the recognized geological formations. But we have nothing to do with these at this time. We have now only to say, that it seems necessary to admit, that when Dr. Emmons insisted on a great group of strata belonging to the age of the lower Cambrian, lying below the Potsdam sand-rock in New York, he had some foundation more substantial than imagination or mere hypothesis.

If we examine the descriptions given by Dr. Emmons of his Taconic system, we shall find that he makes the following broad stratigraphic distinctions: 1°. His highest member is what he designates 'black slate,' which he declares, in some cases plunges apparently beneath the 'ancient gneiss,' and contains a considerable amount of carbonaceous matter. 2°. Under the black slate his next grand distinction was the so-called Taconic slate, which he described as argillaceous, siliceous, and 'talcose;' thickness about two thousand feet. 3°. Below the great mass of soft schists, he described a mass of five hundred feet of limestone, designated 'Stockbridge limestone,' which graduates downward into 'talcose' or magnesian sandstones and slates; the whole having a thickness of about seventeen hundred feet. 4°. Under this limestone is his 'granular quartz' rock, more or less interstratified with slates, and becoming, in some places, an immense conglomerate with a 'chloritic paste.' 5°. The 'ancient gneiss,' on which the Taconic system was said to lie unconformably.

Now, it requires but a glance to perceive how clearly this order coincides with that which has been independently and laboriously worked out in the north-west. We have in both instances a 'black slate,' and below this in both cases is an immense series of soft hydro-mica and magnesian schists. These, again, are followed by crystalline limestone, or marble, which changes downwards to slate, and a hard sand-rock. Below this is the great bed of quartzite; which is, at the base, coarsely conglomeritic with masses of rock from the great underlying series of gneiss.

We are now, however, confronted with another difficulty. The geologists of Michigan and Wisconsin have set aside Dr. Emmons's identification of the Menominee rocks with the Taconic, in 1846, and have called them Huronian. It becomes necessary, therefore, to ascertain of what the Huronian system consists.

The 18,000 feet of the Huronian system on the shores of Lake Huron include 900 feet of limestone, 2,000 feet of 'chloritic and epidotic slates,' and 15,100 feet of quartzite and conglomerate. Perhaps 5,000 feet of this thickness may be considered intrusial. This will leave 12,000 feet, at least, for the aggregate thickness of quartzite and conglomerate, being nearly double that observed in the same horizon in northern Minnesota. It is plain to see, that, if there be any parallelism between these beds and the various groups made out in the north-west, the whole of these strata must be made the equivalent of group 5, or the quartzite and marble group.

There is, therefore, a conflict between the Taconic and the Huronian, both in respect to the horizon which they are intended to cover, and in the horizon of rocks which they actually compass. The Huronian, however, in its original and typical description, can be parallelized with only the very lowest of the strata that were included in the typical and original Taconic; while the Taconic stretches upward at least as far as to include the fourth and third grand groups made out in the north-west; that is to say, the hydro-mica and magnesian schists, and the carbonaceous and arenaceous black slates.

This leaves two series of rocks untouched by the scope of either the Huronian or the Taconic, as these systems were at first defined; namely, the mica-schist group, and the granite and gneiss with gabbro group. In the term 'Montalban,' proposed for these groups by Dr. Hunt, the two are united; and the constant distinctness which they seem to maintain is not recognized. The granite and gabbro group has affinities with the overlying cupriferous rocks, and perhaps, as Irving has suggested, should be considered the base of that series; whereas the mica-schist group has, without exception, been assigned to the same system and age as the underlying groups. The granite and gabbro group has likewise been designated differently. The gabbro has been called Laurentian, Labradorian, and Norian; and the granite and gneiss have received, under one of their modified conditions, the special designation Arvonian. Professor Winchell thought he had already shown that the Arvonian rocks are interstratified with the cupriferous, and are modified sediments of that series. Instead of being near the bottom of the 'Huronian' in the north-west, they overlie all the groups that have been assigned to the Huronian by Irving, and constitute a part of the great series of younger gneisses, which by Brooks has been marked as the 'youngest Huronian.'

It is evident, that at present it is an impossible undertaking, to assign the groups of the crystalline rocks of the north-west to any of the terranes that have been named farther east, without violating somebody's system of nomenclature. Respecting the horizon known as 'Laurentian,' there is an approach to unanimity and agreement. This, however, consists more in a tacit consent to style the lowest known rocks Laurentian, than in any agreement among geologists as to the nature and composition of the strata. The Taconic of Emmons has been generally ignored. The original Huronian has grown from the dimensions of a single group (the quartzite and marble group), so as to include all the crystalline rocks lying above that group, spreading from the Laurentian to the unchanged sediments of the upper Cambrian. This has in some cases become so obviously wrong, and has included groups of rocks so plainly extra-Huronian, that a double and triple nomenclature has been applied to a part of these upper rocks. These new names, with the exception of the name Montalban, seem to be of value only as regional designations; the strata which they represent being igneous or metamorphic, and hence liable to be wanting in some places, and to be non-crystalline in others. They further complicate the stratigraphic nomenclature, since they are probably only the locally modified lower parts of the New-York system.

In conclusion, the chief points brought out in this discussion may be re-stated more concisely:

1. The crystalline rocks of the north-west are comprised under six well-marked, comprehensive groups.
2. The Taconic of Emmons, so named in 1842, and more correctly defined in 1846, included three of those groups.
3. The Huronian of Canada is the equivalent of the lowest of the Taconic groups, and the perfect parallel of only the lowest of the groups in the north-west that have been designated Huronian.
4. The uppermost of the groups in the north-west is local in its existence and exceptional in its characters, and has received, therefore, a variety of names.
5. There are, therefore, confusion and conflict of authority in the application of names to the crystalline rocks of the north-west.

CATAGENESIS; OR, CREATION BY RETROGRADE METAMORPHOSIS OF ENERGY.[1]

THE general proposition, that life has preceded organization in the order of time, may be regarded as established. It follows necessarily from the fact, that the simple forms have, with few exceptions, preceded the complex in the order of appearance on the earth. The history of the lowest and simplest animals will never be known, on account of their perishability; but it is a safe inference from what is known, that the earliest forms of life were the rhizopods, whose organization is not even cellular, and includes no organs whatever. Yet these creatures are alive; and authors familiar with them agree that they display, among their vital qualities, evidences of some degree of sensibility.

After recalling the proposition laid down years ago by Lamarck, regarding the effect on structure of the use and disuse of organs, the speaker explained kinetogenesis as the production of animal structures by animal movements; and archæstheticism as the doctrine that sensibility or consciousness has ever been one of the primary factors in the evolution of animal forms. The influence of motion on development is involved in Spencer's theory of the origin of vertebrae by strains; and the speaker maintained that the various agencies mentioned by Lamarck as producing change are simply stimuli to motion.

In the present address he proposed to pursue the question of the relation of sensibility to evolution, and to consider some of the consequences which it involves; though in the present early stage of the subject he could only point out the logical conclusions derivable from facts well established, rather than any experimental discoveries not already known. Those who object to the introduction of metaphysics into biology must consider that they cannot logically exclude the subject. As in one sense a function of nervous tissue, mind is one of the functions of the body. Its phenomena are everywhere present in the animal kingdom. It is only want of familiarity with the subject which can induce a biologist to exclude the science of mind from the field.

[1] Abstract of an address delivered before the section of biology of the American association for the advancement of science, at Philadelphia, Sept. 4, by Prof. E. D. COPE, of Philadelphia, vice-president of the section.

The hypothesis that consciousness has played a leading part in evolution would seem to be negatived by the well-known facts of reflex action, automatism, etc., where acts are often unconsciously performed, and often performed in direct opposition to present stimuli. But while it is well understood that these phenomena are functions of organized structure, it is believed that the habits which they represent were inaugurated through the immediate agency of consciousness. It is not believed that a designed act can have been performed for the first time without consciousness, on the part of the animal, of the want which the act was designed to relieve or supply. We know, that, so soon as a movement of body or mind has been acquired by repetition, consciousness need no longer accompany the act. The act is said to be automatic when performed without exertion, either consciously or unconsciously; and in those functions now removed from the influence of the unconscious mind, such acts are called reflex. The *origin* of the acts is, however, believed to have been in consciousness, not only for the reasons above stated, but also from facts of still wider application. The hypothesis of archaestheticism, then, maintains that consciousness as well as life preceded organism, and has been the *primum mobile* in the creation of organic structure. It will be possible to show that the true definition of life is, *energy directed by sensibility, or by a mechanism which has originated under the direction of sensibility.* If this be true, the two statements, that life has preceded organism, and that consciousness has preceded organism, are co-equal expressions.

Regarding, for the time being, the phenomena of life as energy primitively determined by consciousness, we may look more closely into the characteristics of this remarkable attribute. That consciousness, and therefore mind, is a property of matter, is a necessary truth, which to some minds seems difficult of acceptance. Clearly it is not one of the known so-called inorganic forces. Objects which are hot, or luminous, or sonorous, are not on that account conscious; so that consciousness is not a necessary condition of energy. On the other hand, in order to be conscious, bodies must possess a suitable temperature, and must be suitably nourished; so that energy is a necessary condition of consciousness. For this reason some thinkers erroneously regard consciousness as a form or species of energy. We all understand the absurdity of such expressions as the equivalency of force and matter, or the conversion of matter into force. They are not, however, more absurd than the corresponding proposition more frequently heard, that consciousness can be converted into energy, and *vice versa.*

The energetic side of consciousness, however, may be readily perceived. Acts performed in consciousness involve a greater expenditure of energy than the same acts unconsciously performed: the labor is directly as the consciousness involved. The dynamic character of consciousness is also shown in its exclusiveness: two opposite emotions cannot occupy the mind at the same moment of time. But there is no fact with which we are more familiar than that consciousness in some way determines the direction of the energy which it characterizes. The stimuli which affect the movements of animals at first, only produce their results by transmission through the intermediation of consciousness. Without consciousness, education, habits, and designed movements would be impossible. So far as we know, the instinct of hunger, which is at the foundation of animal being, is a state of consciousness in all animals.

On the other hand, as consciousness is an attribute of matter, it is of course subject to the laws of necessity to which matter and energy conform. It cannot cause two solid bodies to occupy the same space at the same time, make ten foot-pounds of energy out of five foot-pounds of energy, nor abolish time more than it can annihilate space.

What is, then, the immediate action of consciousness in directing energy into one channel rather than another? Why, from a purely mechanical point of view, is the adductor muscle of the right side of the horse's tail contracted to brush away the stinging fly from the right side of the horse's body, rather than the left adductor muscle? The first crude thought is, that consciousness supplies another energy which turns aside the course of the energy required to produce the muscular contraction; but consciousness, *per se*, is not itself a force (= energy). How, then, can it exercise energy?

The key to many weighty and mysterious phenomena lies in the explanation of the so-called voluntary movements of animals. The explanation can only be found in a simple acceptance of the fact, that *energy can be conscious.* If true, this is an ultimate fact, neither more nor less difficult to comprehend than the nature of energy or matter in their ultimate analyses. But how is such an hypothesis to be reconciled with the facts of nature, where consciousness plays a part so infinitesimally small? The explanation lies close at hand, and has already been referred to. *Energy become automatic is no longer conscious*, or is about to become unconscious. What the molecular conditions of consciousness are, is one of the problems of the future. One thing is certain: the organization of the mechanism of habits is its enemy. *It is clear that in animals, energy, on the loss of consciousness, undergoes a retrograde metamorphosis*, as it does later in the history of organized beings on their death. This loss of consciousness is first succeeded by the so-called involuntary and automatic functions of animals. According to the law of catagenesis, the vegetative and other vital functions of animals and plants are a later product of the retrograde metamorphosis of energy. With death, energy falls to the level of the polar tensions of chemism, and the regular and symmetrical movements of molecules in the crystallization of its inorganic products.

It has been already advanced, that the phenomena of growth-force, which are especially characteristic of living things, originated in the direction given to nutrition by consciousness and by the automatic movements derived from it. There remain, however, some other phenomena which do not yield so readily to this analysis. These are, first, the conversion by animals of dead into living protoplasm; second, the conversion of inorganic substances into protoplasm by plants; and, third, the manufacture of the so-called organic compounds from the inorganic by plants. It is also well known that living animal organisms act as producers, by conversion, of various kinds of inorganic energy, as heat, light, motion, etc. It is the uses to which these forces are put by the animal organism, that give them the stamp of organic life. We recognize the specific utility of the secretions of the glands, the adaptation of muscular motion to many uses. The increase of heat to protect against depression of temperature, and the electricity as a defence against enemies, display unmistakably the same utility. We must not only believe that these functions of animals were originally used by them, under stimulus, for their benefit, but, if life preceded organism, that the molar mechanism which does the work has developed as the result of the animal's exertions under stimuli. This will especially apply to the mechanism for the production of motion and sound. Heat, light, chemism, and electricity doubtless result from molecular aptitudes inherent in the constitution of protoplasm. But the first and last production of even these phenomena is dépendent on the motions of the animal in obtaining and assimilating nutrition; for without nutrition all energy would speedily cease. Now, the motion required for the obtaining of nutrition has its origin in the sensation of hunger. So, even for the first steps necessary to the production of inorganic forces in animals, we are brought back to a primitive consciousness.

To regard consciousness as the primitive condition of energy, contemplates an order of evolution in large degree the reverse of the one which is ordinarily entertained. The usual view is, that life is a derivative from inorganic energies, as a result of high or complex molecular organization, and that consciousness (= sensibility) is the ultimate outcome of the nervous or equivalent energy possessed by living bodies. The failure of the attempts to demonstrate spontaneous generation will prove, if continued, fatal to this theory. Nevertheless, the order cannot be absolutely reversed. Such a proceeding is negatived by the facts of the necessary dependence of the animal kingdom on the vegetable, and the vegetable on the inorganic, for nutrition and consequently for existence. So the animal organism could not have existed prior to the vegetable, nor the vegetable prior to the mineral. The explanation is found in the wide application of the 'doctrine of the unspecialized.' From this point of view, creation consists of the production of mechanism out of no mechanism, of different kinds of energy out of one kind of energy. The material basis of consciousness must, then, be a generalized substance which does not display the more automatic and the polar forms of energy. From a physical standpoint, protoplasm is such a substance. Its instability indicates weakness of chemical energy. The readiness with which it undergoes retrograde metamorphosis shows that it is not self-sustaining. Loew and Bokorny suggest, that "the cause of the living movements in protoplasm is to be sought for in the intense atomic movements, and therefore easy metamorphosis, of its aldehyde groups of components;" the molecular movements becoming molar. The position now presented requires the reversal of the relations of these phenomena. Generalized matter must be supposed to be capable of more varied molecular movements than specialized matter; and it is believed that the most intense of all such movements are those of brain tissue in mental action, which are furthest removed of all from molar movements. From this point of view, when molar movements are derived from molecular movements, it is by a process of running-down of energy, not of elevation; by an increase of the distance from mental energy, not an approximation to it.

The manner in which protoplasm is made at the present time is highly suggestive. The first piece of protoplasm had, however, no paternal protoplasm from which to derive its being. The protoplasm-producing energy must, therefore, have previously existed in some form of matter not protoplasm. In terms of the theory of catagenesis, the plant-life is a derivative of the primitive life, and it has retained enough of the primitive quality of self-maintenance to prevent it from running down into forms of energy which are below the life level; that is, such as are of the inorganic chemical type, or the crystalline physical type.

If, then, some form of matter other than protoplasm has been capable of sustaining the essential energy of life, it remains for future research to detect it, and to ascertain whether it has long existed as part of the earth's material substance or not. The heat of the earlier stages of our planet may have forbidden its presence, or it may not. If it were excluded from the earth in its first stages, we may recognize the validity of Sir William Thomson's suggestion, that the physical basis of life may have reached us from some other region of the cosmos by transportation on a meteorite. If protoplasm in any form were essential to the introduction of life on our planet, this hypothesis becomes a necessary truth.

Granting the existence of living protoplasm on the earth, there is little doubt that we have some of its earliest forms still with us. From these simplest of living beings, both vegetable and animal kingdoms have been derived. But how was the distinction between the two lines of development, now so widely divergent, originally produced? The process is not difficult to imagine. The original plastid dissolved the salts of the earth, and appropriated the gases of the atmosphere, and built for itself more protoplasm. Its energy was sufficient to overcome the chemism that binds the molecules of nitrogen and hydrogen in ammonia, and of carbon and oxygen in carbonic dioxide. It apparently communicated to these molecules its own method of being, and raised the type of energy from the polar non-vital to the adaptive vital by the process. But consciousness apparently early abandoned the vegetable line. Doubtless all the energies of vegetable protoplasm soon became automatic. The plants in general, in the persons of their protist ancestors, soon left a free-swimming life and became sessile. Their lives thus became parasitic, more automatic, and in one sense degenerate.

The animal line may have originated in this wise: Some individual protists, perhaps accidentally, devoured some of their fellows. The easy nutrition which ensued was probably pleasurable, and once enjoyed was repeated, and soon became a habit. The excess of energy thus saved from the laborious process of making protoplasm was available as the vehicle of an extended consciousness. From that day to this, consciousness has abandoned few if any members of the animal kingdom. In many of them, it has specialized into more or less mind. Organization to subserve its needs has achieved a multifarious development. Evolution of living types is, then, a succession of elevation of platforms, on which succeeding ones have built. The history of one horizon of life is that its own completion, but prepares the way for a higher one, furnishing the latter with conditions of a still farther development.

If the principles here announced be true, it is highly probable that *all forms* of energy have originated in the process of running-down or specialization from the primitive energy. One of the problems to be solved by the physicists of the present and future is that of a true genealogy of the different kinds of energy. In this connection a leading question will be the determination of the essential differences between the different forms of energy, and the material conditions which cause the metamorphosis of one kind of energy into another.

That the tendency of purely inorganic energy is to 'run down,' is well known. Inorganic chemical activity constantly tends to make simpler compounds out of the more complex, and to end in a satisfaction of affinities which cannot be farther disturbed except by access of additional energy. In the field of the physical forces, we are met by the same phenomenon of running down. All inorganic energies or modes of motion tend to be ultimately converted into heat, and heat is being steadily dissipated into space.

The process of creation by the retrograde metamorphosis of energy, or, what is the same thing, by the specialization of energy, may be called *catagenesis*. It may be denied, however, that this process results in a specialization of energy. The vital energies are often regarded as the most special, and the inorganic as the most simple. If we regard them, however, solely in the light of the essential nature of energy, i.e., power, we must see that the chemical and physical forces are most specialized. The range of each species is absolutely limited to one kind of effect, and their diversity from each other is total. How different this from the versatility of the vital energy! It seems to dominate all forms of conversion of energy, by the mechanisms which it has, by evolution, constructed. Thus, if the inorganic forces are the products of a primitive condition of energy which had the essential characteristics of vital energy, it has been by a process of specialization. As we have seen, it is this specialization which is everywhere inconsistent with life.

If we consider the relations of the different kinds of energy to each other and to consciousness, it is difficult to draw the line between conscious and unconscious states of energy. One reason is, that, although a given form of energy may be unconscious, consciousness may apprehend the action by perceiving its results. The relations may be expressed as follows: —

A. *Designed* (always molecular).	*Examples.*
I. Conscious.	
1. Involving effort	'Voluntary' acts.
2. Not involving effort	Passive perception. Conscious automatism.
II. Unconscious.	
3. Involving mental process	Unconscious automatic.
4. Not involving mental process . . .	Reflex.
B. *Not designed.*	
I. Molecular.	
5. Electric.	
6. Chemical, 7. Physical,	Crystallific and non-crystallific.
II. Molar.	
8. Cosmic.	

The only strictly molar energies of the above list are the cosmical movements of the heavenly bodies. The others are molecular, although they give rise to molar movements, as those of the muscles, of magnetism, etc. Some molar movements of organic beings are not, *in their last phases*, designed; as those produced by nervous diseases.

The transition between the organic and the inorganic energies may be possibly found in the electric group. Its influence on life, and its resemblance to nerve-force, are well known. It also compels chemical unions otherwise impracticable; thus resembling the protoplasm of plants, whose energy in actively resisting the disintegrating inorganic forces of nature is so well known. Perhaps this type of force is an early-born of the primitive energy, one which has not descended so far in the scale as the chemism which holds so large a part of nature in the embrace of death.

Vibration is inseparable from our ideas of motion or energy, not excluding conscious energy. There are reasons for supposing that in the latter type of activity the vibrations are the most rapid of all those characteristic of the forces. A centre of such vibrations in generalized matter would radiate them in all directions. With radiant divergence the wave-lengths would become longer, and their rate of movement slower. In the differing rates of vibrations, we may trace not only the different forms of energy, but diverse results in material aggregations. Such may have been the origin of the specialization of energy and of matter which we behold in nature.

Such thoughts arise unbidden as a remote but still a legitimate induction from a study of the wonderful phenomenon of animal motion, — a phenomenon everywhere present, yet one which retreats, as we pursue it, into the dimness of the origin of things. And when we follow it to its fountain-head, we seem to have reached the origin of all energy, and it turns upon us, the king and master of the worlds.

MICROSCOPIC SCIENCE.[1]

PROF. T. G. WORMLEY delivered no formal address. He gave only a short discourse, in which he described the advantages and possibilities of two special applications of the microscope: first, to the detection of very minute quantities of certain poisons, notably arsenic, by the examination of the sublimate; second, to the examination of blood stains. He described the limits within which identification of different animals, and the recognition of human blood, is feasible; he denied that human blood can be absolutely identified; he also stated that the result of prolonged experiments indicated that pure water is the best reagent for restoring the blood-corpuscles in a stain to their natural condition.

MAN IN THE TERTIARIES.[2]

IN studying the questions of his own origin and antiquity, man has been hindered by many prejudices and by many barriers of his own erection, the first and most formidable of which was the theological barrier of the Mosaic cosmogony. In process of time this was partially removed; but other barriers to free investigation arose, founded on the evidence collected by the very men who had done most to destroy the earlier obstacles. Cuvier declared that man, being the last and highest of creation, could never have been contemporary with the extinct species of mammals found in the quaternary beds. For a time all evidence to the contrary was treated with contempt; but Cuvier's massive authority was finally overthrown by Perthes, Schmerling, and others.

No sooner had the Cuvierian barrier against quaternary man been demolished, than smaller barriers of precisely the same nature were erected against the tertiaries. Gaudry could not admit that the worked flints discovered by the Abbé Bourgeois in the miocene of Thenay were the remains of men; because he found it difficult to believe, that, while every other species of the miocene is now extinct, man alone should have remained unchanged. Professor Dawkins in a similar line of argument assumes that man cannot be looked for until the lower animals now in existence made their appearance. In the eocene age there were none of the present living genera of placental mammals, in the miocene none of the present living species; and it is most unlikely that man should appear at such a time. At this period the apes (Simiadae) haunting the forests of Europe were the most highly organized types. Moreover, if man were upon the earth in the miocene age, it is incredible that he should not have become something else during those long ages in the course of which all the miocene land mammalia have either assumed new forms or been exterminated. And for similar reasons Professor Dawkins says he cannot expect to find traces of man in the pliocene. But such assumptions are obstructive: they not only put a check upon research, but they prevent the unbiased consideration of fresh evidence.

These theories have been greatly strengthened by the idea that man has been evolved from the higher apes, and that his nearest relations among these creatures are those which are supposed to have appeared last in the sequence. Nevertheless, we find the evidences of man associated with extinct apes, and the gap between them is by no means closed in these earlier horizons. In the earliest remains of man thus far recognized, we do not have the most pronounced ape-like features, as we should have a right to expect if both have sprung from the same stem, and if man is limited to the quaternaries. All these forms are still man, with a fair brain-case; though the slight modifications toward an ape-like structure have the deepest significance in clearly indicating the direction from which he sprang.

If paleontologists are right, the first anthropoid apes have been found in the middle eocene, and later still a more generalized form called Oreopithicus; and side by side with these are found chipped flints if we are to accept the authority of their discoverer Bourgeois and the opinion of Mortillet and others. If man existed then, — and on theoretical grounds there is no reason to believe that he did not exist, — we must look much farther back for the approach of these two groups.

The earliest evidences of man must be sought in his remains, and not in his works; but the very conditions of life which characterized early man and his associates render the preservation of their remains a matter of extreme improbability. The herbivora in herds, seeking the shelter of watery places, would in dying become mired, and thus preserved in a matrix for the future explorer. Aquatic forms are infinitely more abundant as fossils than land or aërial forms, — water-birds than land-birds. The arboreal ancestors of man, and the probable habits of man himself, would leave their bones to bleach in the field or forest, to decompose and disappear long before an entombment was possible. It was only when man acquired the art of sepulture, or sought refuge in caves, that the preservation of his remains became assured. Surface changes, however, have been so wide-spread and profound as to nearly obliterate all trace of these places, and when preserved the harvest from them has been of the most meagre description. Of nearly fifty caves examined by Schmerling in Belgium, only two or three contained human remains. Lund, who examined eight hundred caves in Brazil, found only six containing human remains. The grain of the Swiss lake-dwellers, and even the bread they made, have been preserved; but human bones are of scanty occurrence. The Danish peat-beds have as yet yielded none, though stone implements and other objects are found there in abundance.

Chief among the agencies in destroying the evi-

[1] Abstract of an address before the section of histology and microscopy of the American association for the advancement of science, at Philadelphia, Sept. 4, by Prof. T. G. WORMLEY of the University of Pennsylvania, vice-president of the section.

[2] Abstract of an address to the section of anthropology of the American association for the advancement of science, at Philadelphia, Sept. 4, by Dr. EDWARD S. MORSE, of the Peabody academy of science, Salem, Mass., vice-president of the section.

dences of man have been the glacial floods; and these, if the glacialists are right, have occurred, one during the earlier pliocene and the other at the beginning of the quaternary. In the gradual recedence of the glaciers, no less destructive agencies were at work in scooping out valleys, inundating immense areas, and covering broad tracts of land by their detritus.

It would seem from many facts, that early man lived in the vicinity of water, either on the banks of rivers or along the coast-line; and it is just these regions which have been most profoundly modified since glacial days, and, indeed, in all times.

Saporta suggested the idea that man, originating in the north, had been pushed southward by successive waves of people till the primitive wave was forced into the extremities of the southern continents, and that the remnants of this ancient wave are seen in the Tasmanians, Bushmen, and Fuegians. If so, the remains of primitive man are buried under paleochrystic ice. Far more probable would it be to assume an antarctic continent under genial conditions in which these primitive races lived, and whence successive waves emanated, becoming modified by their new surroundings as they receded from their point of origin. We should then assume the submergence of this region; leaving remnants of these low types in the Patagonians, Tasmanians, Bushmen, and others, and precisely where we might expect to find them. If either supposition is true, the earlier traces of these people are buried beyond recovery. The prejudices of man himself have also caused the loss of much precious material, or of opportunities which can never be regained, — ancient skeletons exhumed only to be promptly buried again; others encountered in excavation, and left undisturbed through superstitious fear. Even at the present time, while the collection and study of the remains of other fossil mammals go on unchallenged, the archeologist is beset by a class who repudiate his facts, look upon his evidences as deceptive or fraudulent, and misunderstand his aims.

From what has been said, it is evident that the discovery of the remains of primitive man is highly improbable. Until this good fortune comes to us, as come it may, we must be content to reason from the known to the unknown. In regard to the physical characteristics of man, there is a manifest disproportion between the changes he may have undergone, and the known change of other mammals since miocene days. For, while slight changes in man's osteological structure have undoubtedly taken place, many mammals of huge form and great variety have become extinct, and others have been profoundly modified. On the other hand, it seems reasonable to believe, that, the moment the ancestors of man possessed the power of banding together in communities, and of using weapons, they became capable of rendering inoperative the very influences which were so active in modifying or exterminating their mammalian associates. How far these conditions were settled in the quaternary, may be seen from the fact, that while man could endure an arctic climate, and survived the glacial period, his anthropoid and more distant pithicoid relations disappeared from Europe forever on its approach.

The fact that man, and his near associates, have been regarded as structurally the highest forms of mammals, has led to the natural belief that they must have been last evolved. That man is pre-eminently the highest form intellectually, goes without the saying; but in regard to his physical characteristics it seems that sufficient importance has never been given to the generalizations of Cope, who shows that "the mammals of the lower eocene exhibit a greater percentage of types that walk on the soles of their feet, while the successive periods exhibit an increasing number of those that walk on the toes, while the hoofed animals and carnivora of recent times nearly all have the heel high in the air. . . . Thus, in all generalized points, the limbs of man are those of a primitive type so common in the eocene. His structural superiority consists solely in the complexity and size of the brain. A very important lesson is derived from these and kindred facts. The monkeys were anticipated in the greater fields of the world's activity by more powerful rivals. The ancestors of the ungulates held the fields and the swamps, and the carnivora driven by hunger learned the arts and cruelties of the chase. The weaker ancestors of the quadrumana possessed neither speed, nor weapons of offence or defence; and nothing but an arboreal life was left them when they developed the prehensile powers of the feet. Their digestive system unspecialized, their food various, their life the price of ceaseless vigilance, no wonder that their inquisitiveness and wakefulness were stimulated and developed, which is the condition of progressive intelligence." This explains on rational grounds why man has continued to persist for so long a time with physical characteristics so slightly modified, while other forms were changing or becoming extinct.

It has been shown that structurally he is related not only to the higher apes, but with numerous lower forms, and even with the lemuroids, remains of which have been found in the lower eocene of both continents. If these structural affinities are valid, then we must look far beyond and below the present higher apes for the diverging branches of man's ancestry.

Another evidence of his antiquity is the early establishment of well-marked types, which must have required an enormous lapse of time to have become established. The various types of skulls are met with among the earliest traces of man. In the lake dwellings of Switzerland, Dr. His has discovered four different types of skulls.

Professor Kollman, who has made an extensive study of the crania of both hemispheres, concludes that the sub-species of man became fixed in the pre-glacial period. Furthermore, the evidences go to show that early man had become sufficiently differentiated to acclimate himself to widely different regions of the earth's surface, while the apes are still confined to the torrid zone. The remains of his feasts show that he had early become omnivorous. The most powerful argument in favor of tertiary man lies in the fact that his earliest remains are not confined to any one region of the earth. The river-drift men are found impartially scattered from tropical India through Europe to North America. If their distribution was by the northern approaches of the continent, it must have been in pre-glacial times, because, as Dawkins shows, an ice-barrier must have spanned the great oceans in northern latitudes.

It seems an almost fruitless speculation, to inquire into the manner of their dispersion, yet one is tempted to surmise that if they originated in the tropics, then submerged continents must again be restored to offer the necessary means for such a dispersal. If, on the other hand, their home was in the north or south temperate zone, and the distribution circumpolar (and this seems more probable), then we have another evidence of the wide separation which the race had acquired, at that early day, from its tropical relatives the apes. Whatever the facts may ultimately show, this unparalleled distribution of a people in the lowest stages of savagery proves beyond question that man must have pre-existed for an immense period of time; for, with the known fixity of low savage tribes, the time required to disperse this people over the whole earth can only be measured by geological centuries.

The farther we penetrate into the past, and ascertain some definite horizon of man's occurrence, other observers in widely different regions of the earth bring to light traces of man's existence in equally low horizons. The evidence of the remoteness of man's existence in time and space is so vast, that, to borrow an astronomical term, no parallax has thus far been established by which we can even faintly approximate the distance of the horizon in which he first appeared. From this fact we are justified in the assumption that the progenitors of quaternary man, under different genera possibly, must be sought for in the tertiaries.

Science will not gain by the erection of any theoretical barriers against tertiary man, until such definite forms are met with that shall reasonably settle the beds in which he first occurred. We know in what rocks it would be obviously absurd to look for his remains or the remains of any mammal. So long, however, as forms are found in the lowest beds of the tertiaries, having the remotest affinity to his order, we must not cease our scrutiny in scanning unbiased even the rocks of this horizon, for traces of that creature who, until within a few short years, was regarded as some six thousand years old, and who, in despite of protest and prejudice, has asserted his claim to an antiquity so great, and a dispersion so profound, that thus far no tendency to a convergence of his earliest traces has been demonstrated.

SCIENTIFIC METHODS AND SCIENTIFIC KNOWLEDGE IN COMMON AFFAIRS.[1]

ECONOMIC science and statistics can hardly do less than to promote the use of scientific methods, and disseminate scientific knowledge in common life. Science has had a hard struggle with ignorance. A host neither small nor amiable has been arrayed against it. What wonder, then, that it has first intrenched itself where the use of instruments of precision and the demonstrations of mathematics separated it from the critical issues of man's everyday conduct? Nevertheless, history may in the remote future express surprise that in America, where the power and conduct of man are so important, science has so long neglected the rugged issues assigned to this section.

There is now no good reason why scientific men should neglect to apply scientific methods to the economy and statistics of every-day life. If mathematical principles and processes are applicable to the statics and dynamics of physics, why not also to the statics and dynamics of society? If useful in economics, why not in personal and domestic life? True, in all questions of conduct, we must include man's free action of will, and leave room for doubt or for alternatives or for contrary choice; yet how many questions of daily life are left to the merest conjecture, to superstition, or to the wild estimaginings, and how large a percentage of blunders might be avoided! We smile that a pagan commander moved his army by the flight of a crow or by the aspect of an animal's entrails; but how many merchants sail their ships, and agriculturists plant or harvest, by the guesses of charlatan weather-prophets, or how many actions are determined by seeing the moon over the right shoulder, or by confidence in a horseshoe! Myriads of groundless notions to-day affect the conduct of personal and public affairs. It is time for science to enter. Many a juggler would then lose his business, many a prejudice have to be given up. Pockets, policies, and politics are involved in the issue. The disposition to revel in the marvellous, to dally with uncertainties, and to treat all mystery as concealing the superhuman, would be disturbed. The phrases 'we guess,' 'we reckon,' are giving way to the phrases 'we will inquire,' 'we will try to know.'

Sir William Thompson has said, "Accurate and minute measurement seems to the non-scientific imagination as a less lofty and dignified work than looking for something new;" but he adds, "Nearly all the grandest discoveries of science have been but the rewards of measurement and patient, long-continued labor in the minute shifting of numerical results." Thus the methods of economic science are the same as those of other branches of science, while the latter also yield statistical results.

It is unfortunate that scientific men aspire so exclusively to original research. We need men to couple love of science with love of mankind. Livingstone desired to explore Africa for science, but as much so for the civilization of benighted Africans. Is science for man, or man for science? Is not benefit to mankind the real measure of the good that is in science?

Doubtless Stephenson was more perplexed with the mood of the parliamentary committee than with the questions of improving his steam-engine. From a

[1] Abstract of an address before the section of economic science and statistics of the American association for the advancement of science, at Philadelphia, Sept. 4, by Gen. JOHN EATON, U. S. commissioner of education, Washington, vice-president of the section.

member of that committee came the absurd question, 'Would it not be a bad fix if the engine should meet a cow on the track?' 'Yes,' said Stephenson, 'it might be bad for the coo.' The dissemination of truth is as scientific as its discovery. Sometimes scientific men act as if truth could not be expressed in the vernacular, — indeed, as if it cannot be truth unless dressed in their terminology. College men used to feel that their triennial would lose character if deprived of the dignity of Latin — though it was often bad Latin. All this foolishness is fast passing away. Already it is an honor to scientifically *teach* science, as well as to advance its domain. Still it is rarely met with, and far less understood than scientific research. Here is a great field for immediate occupancy.

The scientific method of communicating truth recognizes the fact, that in early life man's powers are shaped, and too often the bulk of his knowledge acquired. Hence its fundamental rule must be simplicity in the use of language, and in the presentation of each truth in the concrete. This scientific method is needed even to preserve classic learning from disgrace and disuse. Adopted in the whole domain of scholastic instruction, it would bring new votaries to science, and new benefactors even to the support of pure science. A better taste for all kinds of literature would result. Low writing would be at a discount. We should thus cheapen scientific literature, and increase museums for object-teaching. We may never destroy the taste for low and degrading prints by inveighing against them and thus advertising them, but we may create a taste for valuable reading which will not be satisfied by the vile. This literature cannot be the same for all persons, but the scientific method should pervade it all. Morals would not be excluded, but enforced; the imagination not neglected, but purified and elevated. The body of information could not exclude any truth of service to mankind. Every great subject would bring its contribution shaped to scientific methods and adapted to all minds, — the earth as influenced by the sun and the starry world, its surface of land and water, of mountains and streams and valleys, of barren and productive soil, the plant life that dwells upon it, the animal life it supports, the circumambient atmosphere and its phenomena; and man, the scientific animal who makes all this ado, and for whom it is made, and to whom it is given to possess. The Adam of this period of scientific thought might call up his several sciences, and direct each to yield what it possesses for this correlation of economic thought, for human instruction, guidance, enjoyment, and betterment, for this evolution of science, for the greatest good of man by doing its utmost for the common things of daily life. Gravitation weighs alike the most volatile particle and the vastest of far-off stars. The laws of economic science are the same to the lowly as to the great man: by them he measures the price of his salt, and the safeguards of his liberty.

Towards this gathering-up, for man's daily use, of all the lessons of nature, the progress of the race is tending. Steam, the telegraph, and the telephone focus all thought and action. We shall yet demand of every department of knowledge, 'What good for man?' Each science will have its body by itself, and yet fill numerous relations to every art, and yield its practical lessons to every man according to his understanding and preparation. Data thus correlated will meet the child, — nay, will guide the paternal influence and action in its behalf. But now the child, in its greatest dependency, is met with the destructive follies of ignorance. Neglect, mistakes, or downright violations of nature's laws, often consign him to the grave, or plant in him the seeds of permanent disorder. Physicians may relieve his colic, or cure his disease; but how rarely can they so direct the nursing and training as to assure health! If the impairment is mental, and we go to insane-asylums for advice, we learn what per cent of the cases under treatment could have been prevented, and efforts will be made to cure. But we want prevention, not cure. We want information upon questions of food, of raiment, of shelter, of air, of vocation, of occupation; not for one man, or one class of men, but for all men in all conditions.

The era of this diffusion of knowledge has already actually commenced. Men not engaged in scientific pursuits are gradually coming to feel the necessity of gathering, grouping, and generalizing the data which give them a clearer measure of health, comfort, pleasure, as well as the profit and loss involved. As balance-sheets are studied in business, so are questions of finance, of taxation and public expenditure. Great operations, like those in corn, in coal, in cotton, in wool or silk, leather or lumber, in iron or gold or silver, and of all the great industries, — agricultural, mechanical, commercial, professional, — demand and have their collections of statistics, and their vast accumulations ready as contributions to economic science. But the correlation of all these and their actual results have not yet been reached. Nevertheless, money sees the profits of this wisdom, and is more willing to pay for it. Expert investigators are in demand. Public action requires it. The idea of a republic in which all its citizens shall act patriotically and virtuously, from free choice of the right course and on their own knowledge, demands it. Napoleon I. said, 'Statistics mean the keeping of an exact account of a nation's affairs, and without such an account there is no safety;' while Goethe declared, 'I do not know whether figures govern the world, but this I do know: they show *how* it is governed.' America has accepted the responsibility of reporting its operations, and of disseminating information for the benefit of all the people. Boards of health, of charity, of education, and bureaus of statistics and labor, are demanded by state and nation. They are becoming potent in reducing to order the chaos of data so long without form and void.

The character of the information demanded marks the progress of the age. During how many ages was the counting of men and the measure of their condition undertaken solely to prepare for war! Even our own colonial census was taken for this purpose.

The constitution of our fathers provided for representation in congress and in the electoral college according to population. This has led to vast results. A magnificent world of data is now spread before us by the census. Every man, woman, and child, and their interests, enter into it, and it has its lesson for each in all their various capacities and relations; but not more than a hundred thousand can possess it, and few can master the whole of it. It would be too much to come annually, and therefore cannot be frequent enough to meet every condition. Many statements should be annual. Our system of government affords an excellent opportunity to perfect a system of statistics parallel to the decennial census, and fitted to meet all demands.

Publicists have said much of the importance of the town-meeting as found in New England. An important characteristic of it is the bringing of all questions of public taxation and expenditure and policy to the consideration of all the citizens. This attention of all the citizens to the details of municipal action in large cities is impossible: therefore there are public reports and manifold statements. But should the town system of reports be everywhere adopted, and these be followed by county and state summaries, the nation could group these so as to give a variety of form and result sufficient for each according to his interest. The student and statesman would find them falling into appropriate classes, of sufficient frequency, and in connection with our decennial census of the nation would discover us in the very front rank with respect to knowledge of ourselves as a people. This is now done measurably for the subject of education. Each institution publishes its report or catalogue, most towns and cities their reports; many states gather up the data; and the national bureau, carefully avoiding improper complications, and solely for the purposes of information, issues an annual volume. The result is unique in the history of voluntary statistics. Were this system carried into every other great field, and the whole distilled into a single volume, and should each nation do the same, we should see the beginning of a solid foundation for internationalism, and the scientific method at last pervading the world of thought. It would determine the most far-reaching generalizations, and have an effect upon common life not now possible. Childhood would be ushered into new conditions, and alike the humblest and the highest would more easily find the truth.

BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

PROCEEDINGS OF THE MATHEMATICAL AND PHYSICAL SCIENCE SECTION.

THE session of the British association in Montreal might be fairly designated as a 'section A' meeting, in view of the leading position in British science occupied by the representatives of that section, and the prominence which was accorded them and their section in the general meetings of the association. The retiring president of the association, who was to have been present, but was not, was a distinguished member of the section. His few duties were gracefully performed by another distinguished member of the section, Sir William Thomson, who also presided over the sittings of the section during the meeting. As representing the retiring president he introduced his successor, the president for 1884, in the person of Lord Rayleigh, another of the 'strong' men of section A. Two of the three evening lectures were given by members of the section, on subjects connected with physics and astronomy.

When it is remarked that the place of meeting offers no especial attractions to students of mathematical and physical science, it will be admitted that the roll of the section presented an unusual array of great names, including as it did such as Sir William Thomson, Lord Rayleigh, J. C. Adams, J. W. L. Glaisher, Henrici, Dewar, Preece, James Glaisher, Lodge, Rev. S. J. Perry, Osborne Reynolds, and many others.

As might be easily inferred from a glance at the above list, a large majority of the papers presented had to do with physics rather than with astronomy or pure mathematics. By a judicious action of the sectional committee, and one worthy of imitation, the papers were very fairly 'bunched' by subjects so that one was not required to remain during the entire week in order to listen to the treatment of a particular topic.

The first notable physical paper to be presented was, of course, the address of Lord Rayleigh as president of the association.

This address has already been placed before the readers of this journal, and no extended reference to it will be necessary. Although historical in the main, it was rich in valuable and timely suggestions such as could come only from one as thoroughly familiar with the topics referred to as its author. As a sample of these, may be quoted the remarks concerning the theory of the action of the telephone, which was declared to be "still in some respects obscure, as is shown by the comparative failure of the many attempts to improve it;" and in considering some of the explanations that have been offered, Lord Rayleigh said, "We do well to remember that molecular changes in solid masses are inaudible in themselves, and can only be manifested to our ears by the generation of a to-and-fro motion of the external surface extending over a considerable area. If the surface of a solid remains undisturbed, our ears can tell us nothing of what goes on in the interior."

The address of Sir William Thomson as presiding officer of section A must be carefully read and studied to be appreciated. One or two of the 'steps towards a kinetic theory of matter' may be usefully referred to. The as yet unsurmounted difficulty in the kinetic theory of gases is the explanation of what actually takes place during a molecular collision. It need hardly be said that physicists will not be satisfied until more or less of the obscurity surrounding this subject is dissipated. The mutual action at the moment of collision has been generally assumed to be repulsive by all who have written of or contributed to the kinetic theory. Sir William Thomson asks, May it not, after all, be attractive? Under certain conditions it seems that the appearance of repulsion may be the result of attraction. In general, two molecules approaching each other with a high velocity, assumed to be due to their attraction for each other, will approach obliquely, as the chances of a square 'hit' will be exceedingly small; they will then dash past each other in sharply concave curves around their centre of gravity, and fly asunder again, something, indeed, after the fashion of a comet passing around the sun. "A careless onlooker," says Sir William Thomson, "might imagine they had repelled one another." The idea that this mutual action might be attractive rather than repulsive had been in his mind for thirty-five years, but up to the preparation of this address he had never made any thing of it.

But, after all, the molecules must be infinitely small in order that they may *never* come in actual contact, so that we cannot evade the consideration of the effects of these real impacts when they do occur. Concerning these impacts, but two views seem to be open to us; the one is to imagine the molecule to be a little elastic solid; the other, to conceive it to be a 'configuration of motion in a continuous all-pervading liquid.' It is hardly necessary to say, that, in the opinion of Sir William Thomson, the latter must be the final hypothesis upon which we may rest.

But as a convenient intermediate station he suggested the conception of an elastic molecule, out of which we might not only construct a model of a gas, but, with some satisfaction, by linking these molecules together we might explain the elasticity of a solid. In a paper presented to the Royal Society of Edinburgh in March, 1883, of which the title only had been published, he had shown how an elastic system may be constructed, composed entirely of suitably disposed masses in motion. A system of four gyrostatic masses connected together by links was shown to possess all of the properties of an ordinary elastic spring, although composed of matter in itself entirely devoid of elasticity. By properly linking great numbers of such gyrostatic systems together, a model of an elastic solid results. Such a hypothetical solid lends itself easily to the explanation of such effects as the rotation of the plane of vibration of a wave transmitted through it, as in Faraday's celebrated experiment of the rotation of the plane of polarization of a ray of light in a magnetic field.

Sir William Thomson considered further the possibility of discarding entirely the postulate of rigidity in the materials under consideration, and showed how a hydrokinetic model of matter might be constructed in which all the effects of 'action at a distance' might take place. By means of this the model of a perfect gas might be produced, in which, however, there still exists the difficulty of explaining the case of actual impact of the particles. Some ingenious suggestions were made in the way of surmounting this difficulty; and the whole address was enriched by the most delightful digressions on the part of the author, during which the manuscript was neglected, and the section was afforded the pleasure of following, as best it could, the great physicist in his involuntary excursions into this most interesting but little-explored domain of physical science.

Lord Rayleigh, in his presidential address, had referred at some length to recent investigations concerning the theory of lubricants; and the section was therefore in a favorable mood to listen to the first regular paper on the programme, which was a theoretical consideration of that subject by Professor Osborne Reynolds.

The hitherto unrecognized results obtained by Mr. Tower in his experiments were referred to, Mr. Reynolds undertaking to show that they were in strict accordance with our knowledge of the laws of motion of viscous fluids. Mr. Tower had found, that when the rotating journal with its box was immersed in a bath of the lubricant, the resistance was not more than one-tenth of its value in ordinary oiling, and that the journal was less likely to heat at higher than at lower speeds. By boring a hole through the top of the box, it was found that the oil was forced through with considerable velocity; and on attaching a pressure gauge, as high as two hundred pounds per square inch was indicated. The oil appeared to be carried up by the motion of the journal, and to form a film upon which the box rested. Mr. Reynolds showed that there would necessarily result a difference of pressure on the two sides of the vertical line through the centre of gravity in the thin space between the box and journal; the maximum being on one side or the other, according as the rotation is one way or the other. Mr. Tower had found, that if, after running the journal for some time in one direction, a reversal were made, great heating would result. Owing to the difference of pressure above referred to, it was to be expected that this would occur; as, undoubtedly, the box and journal became adapted to each other for a certain direction of running, and when a reversal was made some time would elapse before a re-adaptation would be completed. This would explain why a new journal and box would *always* heat on first being run, however perfect they may be. Mr. Tower had likened the operation to a stroking of the fibres of the metal in one way by one direction of revolution, and the reverse stroking at the early part of a reversed motion; but this was not the true explanation, as the resistance was evidently a shearing resistance, the sliding of one layer of oil over the other. Sir William Thomson, in commenting upon the paper, called attention to the fact that one solid cannot slide over another without *tearing*.

Professor Reynolds also presented an interesting paper on a method of illustrating the second law of thermo-dynamics by means of kinetic elasticity. If a long flexible cord or chain be suspended with a weight at the lower end, the weight may be lifted a considerable distance by communicating a vibratory motion in a horizontal plane to the upper end of the chain. It then represents an absolutely reversible engine. If the weight, when at an elevated point, be removed from the chain, to straighten the chain out will require as great an expenditure of energy, not counting dissipation through friction, etc., as was consumed in raising the weight. It was shown that in this model the mean square of the velocity of the chain, multiplied by the weight per unit of length, corresponds to the heat in Carnot's engine. Another form of the device consisted of two vertical cords to which a number of horizontal bars of wood were attached at equal distances. In discussing the paper, Professor Fitzgerald described a very pretty illustration of the same principle by means of a 'balanced governor,' with a chain and weight attached in such a way as to be in equilibrium in all positions, the details of which are difficult to describe without the aid of a diagram.

The subject of the relative vapor tensions of a body in the liquid and solid state at the same temperature was discussed in a paper by Professor Ramsay and Mr. Sydney Young.

Professor James Thomson long ago pointed out that there must be a sudden change in the curve of vapor density of water at the point of solidification; and showed that this change was really to be detected in Regnault's results, but that Regnault himself had not thought such a break to occur, and had 'smoothed' his curve at this point; believing errors of observation to be sufficient to cover the discrepancies.

Messrs. Ramsay and Young, by means of ingenious devices, had overcome some of the difficulties of the experimental investigation, and had experimented upon camphor, benzine, water, and several other substances. The results were in accordance with the previously accepted views, and in the case of water were found to agree with those based upon Professor James Thomson's formula.

Radiation was the subject of two or three papers. Professor Dewar offered the methods and results of an investigation of the law of total radiation at high temperatures. The plan and arrangement of the apparatus for the research were ingenious and effective; and Professor Dewar stated that he had just learned from Professor Newcomb, that he had some time before devised and described an arrangement for the same purpose, identical in principle with that made use of in his own work.

For relatively low temperatures, Professor Dewar made use of an iron vessel containing mercury, into which a thermometer-bulb was pushed. The radiation measured was that from one side of the vessel, which was made of exceedingly thin iron; and the heat was received upon the face of a thermopile enclosed in a case properly screened, and arranged so that a steady current of water would be used to maintain constancy of temperature at one face. For these lower temperatures, the equation expressing the amount of radiation was of the ordinary parabolic form, the radiation being nearly proportional to the square of the temperature. The difficulty in dealing with high temperature is, that most substances undergo an alteration in the character of the surface when the temperature is very much raised. The arrangement finally adopted consisted essentially of a platinum air thermometer, the bulb of which was enclosed in a small furnace with a small opening through which the radiation took place. The walls of the platinum bulb were very thick, nearly a quarter of an inch in the actual experiment, and the bulb was connected with a mercury manometer for determining the pressure. Experiments were also made to determine the radiation when the thermopile was protected by an iodine screen. The results were as follows, the numbers being in arbitrary units: —

Radiation at 600°	. 15.5,	screen used,	8.
" " 700°	. 19.5,	" "	12.2
" " 800°	. 29.0,	" "	19.5
" " 900°	. 42.5,	" "	29.9
" " 1000°	. 60.5,	" "	44.0
" " 1100°	. 84.5,	" "	66.0

The assumption was made, that the radiation was represented by some power of the temperature; and this power was found in the first case to be 3.4, and in the second 3.3, thus showing a tendency to approach the fourth power; and attention was called to the fact that many of the results of Dulong and Petit were well represented by the equation $R=at^4$.

Mr. J. T. Bottomly offered a paper on the loss of heat by radiation and convection as affected by the dimensions of the cooling body, and on cooling in a vacuum; which, on account of the absence of the author, was read by Sir William Thomson. The paper was based on an extensive series of resistance measurements of copper wires under various conditions, accepting the well-known coefficient of increase of resistance of copper for increase of temperature. The conclusion was reached, that the emission power was greater for small wires than for large ones, and that it diminished with the pressure.

It would be almost impossible to give a perfectly clear idea of Sir William Thomson's paper on a gyrostatic working-model of the magnetic compass, without quoting the paper entire. What he proposes to accomplish may, however, be readily understood. At the last meeting of the association, at Southport, he had explained several methods for overcoming the difficulties which seemed to have defeated all previous attempts to realize Foucault's "beautiful idea of discovering with perfect definiteness the earth's rotational motion by means of the gyroscope." He had there shown that a gyrostat supported, without friction, on a fixed vertical axis, with the axis of the fly-wheel approximately horizontal, will behave exactly as does a 'magnetic compass,' only with reference to the true or rotational north rather than the magnetic north. A method was there presented for so mounting a gyrostat about such vertical axis as to reduce the friction to a minimum. The present paper was concerned principally in the presentation and discussion of another and simpler plan for realizing the same idea. The plan consisted essentially in suspending a gyrostat, properly constructed, by means of a very long and very fine steel wire attached to a torsion-head, capable of being turned about a vertical axis, at the top. The gyrostat being suspended, by successive adjustments of the position of the torsion-head, a position is found in which the position of the gyrostat, in relation to the torsion-head, shows that the wire is free from torsion. In this position the axis of the gyrostat will be in the true north-and-south line; and, if disturbed from this position, it will vibrate about it precisely as does an ordinary magnetic needle about the magnetic meridian.

The author pointed out several difficulties in the way of the complete realization of the idea, and closed by suggesting some possible methods of mounting, in a simple way, a gyrostat free to move about an axis rigorously or very approximately vertical. Regardless of any practical results which may come from it, the suggestion of a gyrostatic compass is singularly interesting as an example of how *motion* may effectively take the place of a directing *force*, although only one of the many which Sir William Thomson has furnished.

As was naturally to be expected, topics bearing upon the subject of electricity occupied a good share of the time of the section. Unfortunately one or two papers bearing upon this subject had been assigned to the chemical section, and were presented contemporaneously with the electrical discussion in section A. The paper by Professor Frankland, on the chemical aspects of the storage of power, was one which many members of section A would have been delighted to hear. While it was being read, however, section A was engaged in an extremely interesting discussion of the question of the seat of the electromotive forces in the voltaic cell, which was opened by Professor Lodge. For the first time in the history of the association, the experiment was attempted, of assigning a definite topic for general discussion; and the success was such as doubtless to lead to a permanent establishment of the custom. The selection of Professor Lodge to open the discussion was extremely fortunate. He is not only a ready and clear expounder of his own views, but he was fortunate, as a leader in the discussion, in that those views were not those which are generally accepted as being orthodox. His opening paper was largely historical; in fact, too largely so in the opinion of many of his hearers. He traced the history of the discussion from the time of Volta, declaring that the only really great contributions to our knowledge of the subject were those of Volta in 1801 and of Sir William Thomson in 1851. Of late years the so-called contact theory had been generally accepted. This theory, as generally understood, Professor Lodge could no longer accept. He did not believe that two metals in air or water or dilute acid, but not in contact, are practically at the same potential; or that two metals in contact are at seriously different potentials, or that the contact force between a metal and a dialectric, or between a metal and an electrolyte, is small. He did believe that by far the greatest part of the electromotive force of a voltaic cell exists at the zinc and liquid contact rather than at the zinc and copper contact, as generally supposed, although he believed that there was an electromotive force at the junction of every two substances. A summary of the argument may be briefly given as follows, which, as far as it goes, is in Professor Lodge's own words: —

Wherever a current gains or loses energy, *there* must be a seat of electromotive force; and conversely, wherever there is a seat of electromotive force, a current must lose or gain energy in passing it.

A current gains no energy in crossing from copper to zinc, hence there is no appreciable electromotive force there.

When a current blows from zinc to acid, the energy of the combination which occurs is by no means accounted for by the heat there generated, and the balance is gained by the current; hence at a zinc acid junction there must be a considerable electromotive force (say, at a maximum, 2.3 volts).

A piece of zinc immersed in acid is therefore at a lower potential than the acid; though how much lower it is impossible to say, because no actual chemical action occurs.

It was not to be expected that this statement of views, differing so greatly from those usually held, would be received without some protest, and particularly from Sir William Thomson, who has been regarded as the chief exponent and expounder of the metallic-contact theory. Professor Lodge was perfectly successful in inaugurating a discussion which was full of interest; although it can hardly be said to have contributed much to the discovery of a substantial basis of agreement, as he had evidently hoped. Sir William Thomson presented his own views at some length. The subject was one surrounded by great difficulties. He thought there could be no doubt as to a difference of potential at the zinc-copper junction, but the question of the electro-motive force of a voltaic cell might be separated from that of difference of potential at the junctions. He fully agreed with Professor Lodge in his view of the seat of the 'working-force' in the cell. The 'working-force' was essentially chemical force. Undoubtedly, in a certain sense, both the chemical and voltaic or contact views of the question were correct.

Professor Rowland, on being called upon to express his opinion, with characteristic frankness declared that he knew nothing about it. Professor Willard Gibbs called attention to the fact that this was a case similar to several other well-known physical problems, in which an attempt to determine the exact point or place at which a force resides had not been rewarded with success. In such cases much depends upon the standpoint from which the subject is viewed, and it sometimes happened that each of several quite different explanations of a phenomenon might be perfectly correct. This proposition came nearer affording a 'substantial basis of agreement' than any thing else; but it cannot be denied that the impression remained on the minds of many in the section, that the extreme views were nearly, if not quite, irreconcilable with each other. Among the interesting results of the discussion was the somewhat unexpected limitation put upon the generally accepted idea of the 'potential of a body,' by Sir William Thomson. This he defined to be the energy expended in bringing unit electricity from an infinite distance to a point in air extremely near the surface of the body.

Lord Rayleigh described a galvanometer of twenty wires which he had constructed, by joining the wires in multiple arc and also in series, so that the constant of one circuit was exactly ten times that of the other. The instrument was useful for the accurate standardizing of ammeters for measuring currents of from ten to fifty amperes. Professor Shuster discussed the influence of magnetism on the discharge of electricity through gases. He had found that this influence was very different upon the discharge from large electrodes from those usually observed when small electrodes are used. The construction of an apparatus of peculiar form, with large electrodes, had enabled him to obtain many curious effects by the introduction, between the electrodes, of electro-magnets of various forms. He had also found that none of the usual Crooke's effects are produced in mercury-vapor tubes, and this was connected in the theory of the operation with the fact that mercury was a monatomic substance.

In the discussion of a paper by Lord Rayleigh on telephoning through a cable, Mr. W. H. Preece related his experiences in telephoning the Dublin and Holyhead cable, a distance of sixty miles, which had been fairly successful; accurately heard conversation, however, could not be carried on beyond a distance of twenty-five miles. Other experiments had proved that it was at present impracticable to use underground wires in cities for distances of more than twelve miles. In every experiment telephonic circuits were made metallic. With an arrangement of double lines he said he had no difficulty in speaking through two hundred and forty miles on overground wires.

The much-talked-of, and one might justly say the much-abused subject, of the connection of sun-spots with terrestrial phenomena, received considerable attention in a discussion which was opened by Prof. A. Schuster. It is generally agreed that sun-spots have a periodicity; the length of their period being somewhat irregular, varying, indeed, from about eight years to fifteen or sixteen years, but the mean from maximum to maximum being about eleven years. This period might be the resultant of several periods superposed; and Professor Balfour Stewart had pointed out the fact that the irregularity observed could be fairly well accounted for by supposing the superposition of two periods of about ten and a half and twelve years respectively.

The first noticeable effect of this sun-spot cycle was the corresponding cycle in the daily variation of the magnetic needle, — a relation which was also generally admitted. From maximum sun-spot area to minimum sun-spot area, the daily variation of the needle changes in the ratio of about three to two; and in at least two instances brief but violent disturbances in the sun had been known to be accompanied, or at least followed closely, by similarly brief but marked disturbances of the magnetic needle. Such was undoubtedly the case in 1859, as observed by Carrington; and again in 1872, as observed by Professor Young. Professor Loomis has shown that there is an intimate relation between the sun-spot cycle and that of the aurora borealis; and, in fact, the practical agreement of those three cycles — the sun-spot, the magnetic, and that of the aurora — may now be considered as universally admitted. But although much time and great labor have been expended in this direction, it must be admitted that no other connection of solar disturbance as shown in sun-spots, with terrestrial phenomena, has been so completely proved as to command general confidence.

The question of accounting for the magnetic influence had been considered. Were the sun made of solid steel, and magnetized to saturation, it could not produce the effects upon the magnetic condition of the earth which are now justly attributed to it. Whether electricity is conducted in some way or other from the sun to the earth, is a question which cannot at present be answered, although it would be rash to affirm that the space between the sun and the earth does not contain enough matter to conduct electricity. It has been suggested, that variations in the amount of heat radiated from the sun might be shown to be an important factor; and some determinations of the total solar radiation have seemed to indicate that the total amount varied from time to time by as much as eight per cent. But the measurement of the sun's radiation is surrounded by the greatest difficulties, on account of the unknown and possibly varying absorption of the earth's atmosphere. Professor Schuster was convinced that the only mode of attempting the solution of the problem lay in the direction of evading this disturbance by establishing observing stations on the highest accessible points; and he suggested the Himalaya Mountains as offering, on many accounts, the most suitable locality. As the question now stood, he believed he was correct in saying that we know nothing of the variation of the sun's radiation.

The question as to the possibility of investigating the problem, through observed temperature effects upon the surface of the earth, had naturally been considered. In spite of the difficulties surrounding the subject, there could be no doubt that several different observers had proved that a connection existed between the sun-spot period and certain temperature effects upon the earth. Among these effects may be mentioned the agreement between this period and the best wine years on the Rhine; and also with the period of the increasing and decreasing number of cyclones upon the Indian Ocean.

As to a similar period in mean atmospheric pressure, the evidence was very contradictory; but it may be safe to add two other coincidences which seem to be established: the number of small comets about the sun seems to vary through a period about in agreement with that of the sun-spots; and it has been shown, particularly in photographs secured by Professor Schuster himself, that the appearance of the solar corona depends in some way on the same cycle.

Professor Schuster was followed by Mr. W. Lant Carpenter, who read a paper upon the same subject, prepared by Prof. B. Stewart and himself. It consisted, in the main, of a description of some very elegant methods which they had made use of in discussing the temperature observations of Toronto and Kew, for the purpose of detecting short periods common to solar and terrestrial phenomena. One of the results of this investigation was to show, that, in general, temperature phases make their appearance at Toronto eight days before they appear at Kew; while what might be called 'magnetic declination range weather' travels from Toronto to Kew in about one and six-tenths days.

The subject was further discussed by several members of the section, among whom was Rev. S. J. Perry, who took a very conservative view of the matter, and declared that much research was demanded before any thing really definite would be known.

The sun, at least as to its spectrum, received further attention from Professor Rowland and Rev. S. J. Perry, — the former exciting great interest in the section by an exhibition of several of his latest spectrum photographs, and a discussion of the remarkable advances in this direction which had followed, necessarily, the use of his diffraction gratings. Mr. Perry's paper was a discussion of observations on the spot spectrum from D to B.

Professor Carpmael described a new form of induction inclinometer which he had devised, which was a modified form of Lloyd's instrument, a bifilar suspension being substituted for an unifilar, and one or two other changes made. The instrument had only been in use a few weeks, but it promised to be of considerable value.

The Earl of Rosse described his method and machinery used for polishing specula, and with which he had completed at Parsonstown a three-foot and a six-foot speculum. He also described a device for securing electrical control of an equatorial driving-clock, which he had recently tried, and found to be very satisfactory. It was essentially a 'see-saw' escapement, with a piece of soft iron on an extension at one end, which moved between two electro-magnets, being held firmly by each, during a certain portion of the swing of the controlling pendulum. In this way, he had secured accuracy and certainty of control, even with crude apparatus.

Mr. Perry, in speaking of the great importance of accurate control of an equatorial, now that the spectroscope had come so to the front, said that it was interesting to know that Mr. Huggins, in producing some of the most perfect telescopic photographs that had yet been made, had not especially felt the need of a more perfect controlling device; since Mrs. Huggins was constantly at his side to regulate the position of the instrument, and that his splendid results were largely due to her precision and patience.

One of the most interesting and novel papers was that of Professor Douglass Archibald, describing his method of sending anemometers into the air by means of kites, and thus studying the velocity of the air at different heights. The carrying kite was seven feet in length; and this was raised and afterward somewhat steadied by a smaller one about four feet long attached to it. The anemometers were arranged at different points along the line of the larger kite, so as to record the velocity at various heights, in some cases extending up as high as six hundred feet. Although the experiments thus far made were only preliminary in their character, some interesting results had been obtained. The velocity increased with increased height, but at a diminishing rate. On being questioned, Professor Archibald declared that the most important thing about a kite was its tail. In his kite the tail was made up of cones of canvas arranged with their bases towards the wind, with the cord running along their axes. They were placed at a distance of three or four feet from each other, and six were used. Sir William Thomson said that after more than a century the kite was again being dedicated to science, first on one continent, and now on another. He was convinced that the device of Professor Archibald was sure to prove to be of great value in meteorological research.

Professor Archibald made a brief reference to the work already accomplished by a committee, of which he was a member, known as the 'Krakatoa committee.' Their object was to determine, if possible, whether the sun-glows or remarkable sunsets of the past year could in any way be attributed to the general diffusion of dust from the eruption of that volcano. They had succeeded in collecting much information, which had not yet been examined; and he could only say that nothing had yet appeared which was inconsistent with the Krakatoa theory.

Some further contributions to meteorology were made in a paper by Professor James Thomson on whirlwinds and waterspouts; in a note on internal earth temperature by Mr. H. S. Poole, in which the increase in temperature at Wolfville, N.S., was shown to be in fair agreement with other well-known determinations; and in a paper by Dr. H. Muirhead on the formation of mackerel sky. The latter was an extension of the explanation suggested many years ago by Sir William Thomson, by the introduction of a third stratum of moving air. The effect of one stratum moving over another, as Sir William Thomson had suggested, would be to produce 'waves' in the air, which might result in long lines of cloud-forms. A third stratum, moving in a direction at or near a right angle to the first, would tend to break these lines up into small patches, thus producing the peculiar appearance known as the mackerel sky.

Prof. Chandler Roberts interested the section greatly in presenting the results of some experiments which he had carried out to show the diffusion of metals; the cases specially considered being the diffusion of gold, silver, and platinum in lead. The rate of diffusion in these cases, and notably in the case of gold, seems to be enormously high compared with the rate of diffusion in liquids.

Mr. W. J. Millar read a paper on iron and other metals in a liquid and solid state, which started a lively and entertaining discussion of the question of the expansion of iron on solidification. Mr. Millar contended that iron did not expand on solidification; while Sir William Thomson, and other members of the section, protested that Mr. Millar's own experiments proved conclusively that it did.

The matter of the velocity of light of different colors was considered by Professor Michelson, and also by Professor George Forbes. Mr. Michelson explained, somewhat in detail, his method of determining the velocity of light, and gave the results of an investigation of the velocity of red and blue through a column of carbon bisulphide about ten feet long. The velocity of the mean ray through this medium had been found to be about 1.75 times its value in air, which was somewhat higher than theory would indicate; but the difference was doubtless attributable to errors in experiment. A measurable difference between the velocity of the red and that of the blue ray had been observed, agreeing very closely with that indicated by theory. Professor Forbes's paper was a discussion of the observations by means of which he, in junction with Mr. Young, had shown, apparently, that there was a measurable difference between the velocities of red and blue light in air. The paper was discussed by Sir William Thomson, Lord Rayleigh, Professor Newcomb, Professor Michelson, and several others; and the general opinion was quite decidedly against the view that such difference really existed.

On the last day of the session, the section was divided; and a number of papers on pure mathematics occupied the attention of a sub-section. No report of these papers can be made, as the *Science* reporter found it impossible to organize a sub-section to follow the mathematicians.

PROCEEDINGS OF THE SECTION OF CHEMICAL SCIENCE.

THE session opened at noon, Aug. 28, with the president, Sir Henry E. Roscoe, in the chair. Dr. Perkins, the retiring president, sat on his right hand; and Drs. Wolcott Gibbs, Gladstone, and Frankland, on his left. The room was filled to overflowing; and the address was listened to with marked attention and interest, and the comments upon it were uniform in their commendation. This is rather surprising when we recall the present state of feeling in England which the efforts to found a superior institution for technical instruction have aroused, but his views on chemical education are in conformity with those generally entertained in the United States. It will be seen by the papers presented at this session, that the particular phases of the recent advances in chemistry of which the president treated occupy at present the attention of many of the English chemists.

The first paper read was by Dr. Wolcott Gibbs, at the request of the section, and was upon the complex inorganic acids. It consisted of a *résumé* of the magnificent work which he has done in the field which he has discovered and explored.

It is impossible in the brief space at our command to do justice to this superb research; which is destined to revolutionize many of our chemical conceptions, and in which has been shown the cumulative power of the molybdenum and tungsten oxides, the existence of dominant and subdominant groups, and of different kinds of basicity prevailing within the same molecule, and of the production of isomerism by the orientation of the atoms.

Mr. H. B. Dixon exhibited tables in which Bunsen's, Horstmann's, and his own results on the effect of mass on the incomplete combustion of mixtures of carbon monoxide, hydrogen, and oxygen were compared; and the discrepancies were found to be due to differences in the temperature and pressure under which the experiments were conducted. Above four hundred millimetres, the pressure did not affect the results; and at temperatures between 60° and 140° constant results were also obtained. It is believed, that when the mixtures were exploded below 60°, the reaction was interfered with by the condensation of water on the sides of the tube. Further, it was found that mixtures of carbon monoxide and oxygen, in equivalent proportions, could not be exploded unless there were aqueous vapor, or some body containing hydrogen, present. With traces of hydrogen, hydrochloric acid, hydrogen sulphide, or a hydrocarbon present, the mixture could be exploded. It is supposed that the steam is reduced by the carbon monoxide, and that the liberated hydrogen burns, and re-forms steam, which again acts on more carbon monoxide. By a series of alternate reductions, a few molecules of steam serve to carry oxygen to the carbon monoxide just as the oxide of nitrogen acts in the sulphuric-acid chamber. By putting a dry mixture of carbon disulphide and oxygen into a dry mixture of carbon monoxide and oxygen, the first could be inflamed, then by introducing a little water the carbon monoxide and oxygen could be exploded.

Professors Liveing and Dewar read a paper on the spectral lines of the metals developed by exploding gases. Berthelot has recently investigated, by means of the chronograph, the rate of propagation of the explosion of mixtures of oxygen with hydrogen and other gases; and has found, that, with a mixture of hydrogen and oxygen in the proportion to form water, the explosion progresses along a tube at the rate of 2,841 metres per second, a number which is not far from the velocity of mean square for hydrogen particles, on the dynamic theory of gases, at a temperature of 2,000°.

This velocity, though far short of the velocity of light, bears a ratio to it which cannot be called insensible. It is, in fact, about $\frac{1}{100000}$ part of it. Hence, if the explosion were advancing towards the eye, the waves of light would proceed from a series of particles lit up in succession at this rate. This would be equivalent to a shortening of the wave-length of light by about $\frac{1}{105000}$ part; and, in the case of the yellow sodium lines, would produce a shift of a distance of about $\frac{1}{107}$ of the space between the two lines. It would require an instrument of very high diffusive power and sharply defined lines to make such a displacement appreciable. With lines of longer wave-length, the displacement would be proportionately greater; while, if a receding explosion could be observed simultaneously with an advancing one, the relative shift would be doubled. In this way the two images of the red lithium line would be separated by about $\frac{1}{8}$ of a unit of Angström's scale, a distance about equal to that between the components of the less refrangible of the pair of E lines.

The experiments were made first in a straight glass tube, and then in a U tube, which enabled them to observe the advancing and retreating wave. In these cases it was found that the calcium spectrum was produced, owing to particles of the glass detached by the explosive reaction. The reversals showed too, that, in the wave of explosion, the gases do not reach their maximum temperatures all at once, but the front of the wave is cooler than the part which follows and absorbs some of its radiations, while the rear of the wave does not produce the same effect.

Experiments were now made in iron tubes, and here the spectrum of iron was obtained from the particles detached from the tube. Altogether, sixty-eight lines of iron were identified, of which about forty lie in the ultra-violet between hydrogen and oxygen. Only one iron line above oxygen was definitely seen, and that in only a few photographs. Since iron gave so many lines, linings of copper, lead, cadmium, zinc, aluminium, and tin were inserted in the tube. Cadmium, aluminium, and tin gave no lines whatever; zinc gave only a doubtful impression; lead gave one visible line, and two in the ultra-violet; copper gave one visible line in the green, two in the ultra-violet, and occasionally a shaded band in the blue; cobalt and nickel gave a great many lines. Berthelot and Vielle having put the temperature produced by the explosion of hydrogen and oxygen under a pressure of 9.8 atmospheres at 3,240°, the authors believe that they cannot be far wrong in assuming the temperature at about 3,000°, and that at this temperature such metals as iron, nickel, and cobalt are vaporous, and emit many characteristic rays, and that by far the greatest part of these rays lie between G and P.

The discussion on the constitution of the elements was opened by Dr. Dewar; and after referring to the doctrine of continuity found in the essays of Grove, taught by Black, and held by Newton, and the views of Clerk Maxwell who said that the process by which atoms are formed cannot be known, since they are neither born nor do they die, he stated that our recent knowledge on the constitution of molecules was largely due to the studies of Deville upon dissociation, and that he was led to make these studies from the observation of Grove that a platinum bead heated in an oxyhydrogen flame would decompose water when immersed in it. Experiments made by Dr. Dewar in this direction were described; and it was stated that chemical bodies are not fixed or unstable at certain fixed temperatures, but that there exists a relation between the pressure, temperature, and character of the body, which determined its stability. Deville held the change to be similar to a change in state of bodies; and, this relation being true, thermodynamics enable us to determine the amount of change for given conditions. The change of state in elementary substances is not unlike a chemical change.

The spectroscope has been used to study the constitution of molecules, and Roscoe has found that the allotropic forms of bodies give different spectra. Lockyer has attempted to show the evolution of the elementary bodies from hydrogen. His results have been criticised as having been due to the presence of impurities, but Lockyer disproved this. It has been said, too, that he did not use a spectroscope of sufficiently dispersive power.

Prout's hypothesis was next considered; and it was shown that the most careful determinations of the atomic weights of nitrogen, potassium, magnesium, zinc, and bismuth, by Stas and Marignac, yielded results that were not simple multiples of hydrogen.

In continuing the discussion, Dr. Gibbs said he was not sure that the accepted views of the molecular constitution of chemical compounds was the correct one. Taking common salt, for instance, it might in the solid state be composed of one hundred molecules of sodium and one hundred molecules of chlorine; when in solution it might be simpler; when in the gaseous form, simpler still; and when exposed to a vacuum, such as Mr. Crookes has produced, it might have the accepted constitution. He referred to the fact, that Professors Liveing and Dewar had found that cadmium, mercury, and zinc gave no spectra at high temperatures. As these are all monatomic molecules, it might be that in this process we possessed a means for studying the constitution of the elementary molecules. Professor Liveing thought Dr. Gibbs's suggestion concerning the action of the monatomic elements an improbable one, since aluminium and tin gave no lines under the same conditions. He said that many lines of iron suggested either a very complex constitution, or else that the substance we term iron is really formed of a number of elements which yet defy separation, and which have nearly similar atomic weights. We have an instance of such a case in the cerium group. The D_3 line, for instance, may belong to an element more volatile than hydrogen. Sir Lyon Playfair pointed out, that when solid iodine was immersed in liquid sulphurous acid no action resulted; but if the iodine was in solution, and the sulphurous acid gaseous, they combined readily. He suggested the study of the temperature at which iodine or sulphur would combine with sulphurous acid. Dr. Tilden said we needed more extensive and accurate observations on the temperature at which chemical action — such, for instance, as the point of ignition — begins. Dr. Dewar stated that we have, in the result of the researches of Dr. Perkins in the magnetic rotation of compounds in relation to their chemical composition, a means for determining molecular weights by optical methods.

The reports of the committees on spectrum analysis and on chemical nomenclature will be published in full, in the annual report of the association.

A paper was next read on some phenomena of solution illustrated by the cases of sodium sulphate by William A. Tilden. In a recent paper in the Philosophical transactions, the author has favored the theory which ascribes solution, not to any combination, chemical or otherwise, of the solid with the solvent, but to liquefaction arising from the mechanical or kinetic action of the molecules of the liquid in which the solid is immersed. This theory is now being tested through a study of the thermal phenomena attending the solution of sodium sulphate.

Crystallized sodium sulphate containing ten molecules of water melts at 33°-34°. At 34° or thereabouts it begins to show signs of dissociation. The maximum point of solubility likewise is at this temperature. In consideration of these facts, Professor Tilden propounds the query: In what condition is the dissolved salt at temperatures above 33°? Is it in the form of the usual hydrate, or is it wholly or in part in the anhydrous state? The diminished solubility is believed to indicate progressive dissociation; but, this view being questioned, the heat of solutions at temperatures above and below the critical temperature is being determined. The data given, although subject to some slight revision, show that at temperatures as high as 55° the thermal change is still positive, although a diminishing quantity; and hence, that the act of solution is still attended at these temperatures by a chemical combination between the salt and a portion of the water. In this connection, Professor Tilden presented a modified form of calorimeter used in his experiments.

W. W. J. Nicol next presented a theory of solution. The theory proposed is, that the solution of a salt in water is a consequence of the attraction of the molecules of water for a molecule of salt, exceeding the attraction of the molecules of salt for one another. It follows, then, that, as the number of dissolved salt molecules increases, the attraction of the dissimilar molecules is more and more balanced by the attraction of the similar molecules: when these two forces are in equilibrium, saturation takes place. At the saturation point the force tending to keep in solution any single molecule of salt (attraction of dissimilar molecules) is balanced by the force tending to produce separation of that molecule from the solution (attraction of similar molecules). Further, any external cause tending to alter the intensity of either of these two forces, or to modify both in unequal degrees, disturbs the condition of equilibrium, and further solution or solidification ensues. The above theory is based on the molecular theory of liquids, and has many points in common with that of Dassios proposed in 1866.

In putting this theory to the test of experiment, certain results followed which in such a brief note as this cannot be mentioned.

Mr. Nicol lays stress upon the fact that he expresses the value of a salt solution by n molecules (equivalents) of salt to one hundred molecules of water; and he holds that the experiments made on the continent are valueless where they have been made by dissolving one, two, or more molecules of salt in a litre of solution, since, as the molecular volumes of the salts in solution vary, the solutions are not similar as supposed.

A paper followed on evaporation and dissociation, by Professor William Ramsay, and Sydney Young. It having been suggested, that the closer proximity of molecules in the liquid and solid state may be due to the coalescing of two or more gaseous molecules, to form a complex molecule, the authors hold that the work done in dissociating these complex molecules into single molecules is analogous to that expended in converting a solid or liquid into gas, and that the same relations between the existing temperature and pressure would exist. The temperature of volatilization of a large number of solids was determined by the 'cage' described by them before the Royal society, April, 1884. With bodies like phthalic and succinic acids, this relation was found to exist; but with acetic acid little or no dissociation was discovered. Also a distinct difference was observed in the behavior of dissociating substances in the liquid and solid states when evaporating from a full surface. So long as a substance is solid, the residue retains its original composition, but a liquid separates into its components: this amounts to a proof that a solid in volatilizing does not pass through the liquid state, and that so long as a substance remains solid it cannot dissociate. The results obtained lead the authors to provisionally doubt the existence of complex molecular groups in liquids.

The object sought in Professor William Ramsay's paper on molecular volumes was to ascertain whether the boiling-points of compounds, under equal pressures, really afford suitable points for a comparison of the molecular volumes. The experiments made decisively show that in methyl, ethyl, propyl, isopropyl, and isobutyl alcohols, and ether, the value of the group CH_2 is by no means constant: while at the boiling-points of the liquids at low pressures, the value is approximately constant, fluctuating between 17.5 and 22, at high temperatures the difference becomes much more apparent, attaining at pressures of 20,000 mm. (which was the highest measured) the greatest irregularity.

Professors Goodwin and Marshall are studying the solubility of chlorine gas in solutions of metallic chlorides; and finding that other experimenters have been observing the expansion of solutions made by dissolving n molecules of the salt in m molecules of water, and that consequently these contain, when diluted, neither the same number of molecules of the salt nor of the water, they have arranged their experiments so that this ratio shall remain constant throughout the observations.

Sir H. E. Roscoe, speaking in regard to the diamantiferous deposits of South Africa and the ash of the diamond, showed that silica and iron oxide form constant constituents of the diamond. It is a curious fact, that when these yellow diamonds are heated out of contact with the air they lose their color, and remain colorless so long as they are not exposed to the light: then they immediately regain it.

A discussion on chemical changes in their relation to micro-organisms was opened by Professor Frankland. He stated that contact action had been held to be of two kinds, — that where both of the bodies underwent a change, and that in which one of the bodies remained unchanged. The last was called catalytic action. The changes taking place in organized bodies had been referred to the last class, but organic chemistry had proved them to belong to the first. In organized bodies, both analytical and synthetical changes take place; but in general the first take place in the bodies of animals, and the last in vegetables. This enables us to determine to which of the two kingdoms a body belongs, and judged by this criterion the microcosms belong to the animal kingdom. Soluble ferments, on the contrary, act by contact without giving of themselves. The changes which these soluble ferments produce were then shown in a series of tables; and it was seen that the resulting analytical reactions were usually quite simple, but were attended by the evolution of heat. Referring to this point, it was suggested, that as allotropic and isomeric changes often convert potential into kinetic energy, it might be possible to maintain life through these changes. The reactions produced by the micro-organisms were next shown in a series of charts, together with illustrations of their forms. The reactions in these cases were far less simple; but in some instances, as with the Saccharomyces cerevisiae, it is a question how far the by-products are due to the action of the micro-organism. The power of these organisms to resist chemical substances generally and high temperatures was shown, yet spongy iron quite destroyed them. It is of the utmost importance to discover some simple agent for destroying these bodies, which is harmless to man.

In discussing this topic, Professor Roscoe pointed out the fact that one ferment produces only one reaction, and that this was probably true in those more complicated reactions which attend disease. Dr. Dallinger stated that he was able by slow stages to so change the environment of a micro-organism, that eventually it lived under conditions entirely unlike its natural ones, and that he had cultivated the most highly organized ones in solutions which contained no organic matter whatever. Dr. Dewar called attention to the wonderful preservative power of hydrogen peroxide. One one-hundredth of one per cent will preserve urea indefinitely. It does not, however, preserve milk indefinitely, on account of the physical action of the milk globules, while it has no action whatever on the soluble ferments. He believes the heat evolved by the action of the ferments to be due to the hydration of the alcohol; and he pointed out that we have in bacteria the most delicate agent we now possess for detecting oxygen, and the most accurate for measuring light.

Sir John Lawes and Dr. Gilbert presented a paper on some points in the composition of soils. This was a continuation of the paper presented to the American association two years ago; and it is sought to show that the view which has been maintained, that a soil is a laboratory and not a mine, is erroneous; for not only the facts adduced by the authors in this and other papers, but the whole history of agriculture so far as we know it, clearly show that a fertile soil is one which has accumulated within it the residue of ages of previous vegetation, and that it becomes less fertile as this residue is exhausted. The results of many analyses and experiments with the soils of Manitoba and other prairie lands were cited in evidence.

PROCEEDINGS OF THE SECTION OF GEOLOGY.

It is impossible, in the limited space at our disposal, to do any thing like justice to the large number of interesting papers presented to this section, and to the discussions called out by them. Moreover, coming prominently before the section as there did, such questions as glacial action, causes of the ice age, formation of the basins of the great lakes, the origin of coal, metamorphism, and the many questions connected with the archean rocks, and when these questions were discussed by men like Dawson, Hall, Geikie, Newberry, Hunt, Bonney, and by many younger though no less earnest workers in geology, it is easier to imagine than to describe in detail the interest attached to such an occasion.

The number of papers presented — fifty-one — was too large to admit of satisfactory discussion; and, even hurried over as they were, it was necessary for the section to meet again upon a fifth day, instead of completing its work in four sittings as was originally anticipated. Many of the topics presented were passed over so lightly as rather to discourage the presentation of papers containing the results of long and patient labor. Even the important questions treated of by Dr. Blanford in his opening address were lost sight of except as he occasionally called them to mind.

While the discussions were sufficiently animated, — some of them perhaps even more so than was seemly, — the animation was due, to a considerable extent, to the tenacity with which each one held to his own theories, rather than to any considerable array of facts brought forward to sustain them.

The section met in the lecture-room of the Redpath museum. A full audience heard the address of Dr. Blanford the chairman, and toward the close of its delivery Lord Lansdowne was one of the listeners.

At the close of the address, in accordance with English usages, a vote of thanks to the speaker was proposed by Sir William Dawson, who commended Dr. Blanford's presenting a subject so full of debatable matter as likely to excite the greatest interest and discussion. Seconding the motion, Dr. Selwyn, director of the Geological survey of Canada, referred to instances similar to those mentioned, which occur in Vancouver's Island and in parts of Australia not referred to in the address by the chair. The general impression made by the address seemed to be that the problems presented were not only important ones, but too much so to admit of much discussion here, and that they can be solved only by a large amount of observation and field-work.

The ten papers presented during the first day's session, with one exception, related to the geology of the dominion.

The paper of Mr. Gilpin upon gold-mining in Nova Scotia, that of Mr. Brown upon the apatite deposits of Quebec, and that of Mr. Merritt upon the localities and output of economic minerals in Canada, were more or less statistical, and, although important in themselves, did not admit of much discussion.

A short paper by Mr. Frank Adams of the Geological survey of Canada, upon the occurrence of Norwegian 'apatitbringer' in Canada, and its associated minerals, although upon a subject mineralogical rather than geological, was a valuable contribution in itself, and drew forth an interesting discussion by Dr. G. H. Williams of Johns Hopkins University. Recent studies of optical anomalies seen in many minerals seem to show that not a few substances have different crystalline forms at different temperatures. One of these, pyroxene, has a tendency to pass into hornblende when the temperature is lowered. Nature may accomplish the same thing by pressure. Such changes have been observed by Dr. Williams in certain rocks in Maryland and New York, where schistose structure and these changes appear to be co-extensive.

Mr. Honeyman's paper upon the geology of Halifax Harbor was strongly dissented from by Dr. Selwyn, supported by Professor Hitchcock, who insisted that the rocks at that locality were lower Cambrian, and not archean as stated, except perhaps in isolated masses.

The coal-fields of the dominion were treated of directly and indirectly by Mr. Bailey of the Canadian survey on the Acadian basin in American geology, Mr. Gilpin on the distinctive features of the Nova Scotian coal-field, and by Mr. Budden on the coals of Canada.

The Acadian basin borders upon and includes the Gulf of St. Lawrence, New Brunswick, Nova Scotia, Newfoundland, and Prince Edward Island, dipping on all sides toward the gulf. Within this great basin, the most important coal-fields are those of Cumberland, Pictou, and Cape Breton. The beds are more or less folded; the axes of the folds are east-west; and, except where they have been complicated with older strata, there are no serious faults. Differences between districts within this great basin are probably due to local influences in the original basin, rather than to isolation. Attention was called by Dr. Selwyn to the contrast between this broken region and the less-disturbed country adjoining it to the west and north-west, which he considered was due to the limiting of the disturbances by the great St. Lawrence and Champlain fault. This fault is supposed to follow up the St. Lawrence River from somewhere in the gulf, to Quebec, where it leaves the stream, and swings more strongly to the south, and passes down Lake Champlain to somewhere in the vicinity of the Hudson River. To the east of this great fault, the rocks are metamorphosed, folded and broken, while to the west they are but slightly disturbed, and dip at low angles. Besides the coal-fields occurring in the St. Lawrence basin, the two other localities within the dominion producing coal were referred to; one extending from the 97th parallel to the base of the Rocky Mountains, the other on Vancouver's Island. Of the three fields, the first is in the carboniferous, while the last two belong to the secondary or tertiary formations. But little is known as yet of the coal of north-west British Columbia, while that of Vancouver's Island is said to be the best on the west coast.

Mr. Panton's contribution upon the Silurian strata of Red River Valley, Manitoba, was of local interest, and referred to a structure that is not indicated upon the new geological map of the dominion, for want of sufficient information. The same material is already in the hands of the Canadian survey, and will appear in due time.

The principal discussion of the first day's meeting was called forth by Professor Claypole's paper upon the crumpling of the earth's crust as shown by a section across Huntingdon, Juniata, and Perry counties in Pennsylvania, a distance of sixty-five miles. The speaker showed that the folding of the strata along this line, and especially of those in Cumberland valley, has caused a shortening from an original length of about one hundred miles to the present sixty-five miles. Although Professor Claypole's method of obtaining the original length of this line was a mathematical one, and though the folding of the Cumberland-valley strata is a series of overturns, such an extensive contraction of the earth's crust was more than the section was prepared to accept without question. Doubt was expressed in regard to the trustworthiness of the data; while another member, in endeavoring to solve this very problem, basing his estimates upon Professor Lesley's maps, had computed a contraction of eighteen miles over a part of the section where Professor Claypole had made out thirty-two. It was also suggested that the thinning of the beds by crushing in the folded parts had been left out of account.

It was replied to these objections, that the data were as trustworthy as it was possible to obtain; that absolute accuracy was not claimed for the figures, for in such a case it was impossible; and that, at the least estimate, the eighteen miles remained to be accounted for over one part of the section line. The possible thinning of the beds had been left out of account, because, if such a thing had taken place in this instance, it was more than counterbalanced by the tangential pressure that caused the folding.

The 29th was devoted to the discussion of phenomena relating to, and supposed to be the results of, glacial action. Professor Lewis spoke upon the marginal kames of Pennsylvania as distinguished from the moraine; and Dr. Newberry followed with a short lecture upon the last phases in the evolution of the

North American continent. He pointed out the evidences of a genial climate at the close of the miocene and pliocene, which were soon followed by the age of ice; traced the southern limit of the ice-sheet across the continent as far as it has been observed, and expressed his belief in two glacial periods.

These papers were discussed together. Professor James Geikie was unable to draw any sharp line separating moraines and kames, for they merge into each other in such a manner that one cannot say where one leaves off and the other begins. Kames he regarded as partly morainic, and partly of subglacial origin; and he was in accord with Dr. Newberry in regard to the break in the glacial period. Sir William Dawson was inclined to think that water was largely instrumental in producing the work attributed to ice, and referred to the evidences in eastern North America, of the warm interval during the ice age. Dr. Selwyn briefly proposed a possible explanation of the supposed power of ice to excavate, in solid rocks, basins like those of the great lakes. He referred to the profound decomposition of rocks observed in Australia and in the gneiss of Brazil. In Australia this decomposition was sometimes two hundred and fifty feet deep; and he thought it possible that ice, entering such a region, would be able to make such basins as those of the great lakes. Professor Spencer, of the university of Missouri, contributed some of the results of his own work upon the subject of the origin of the basin of Lake Ontario, which led him to believe that this lake basin, at least, was not of glacial origin. Professor Hall of New York called attention to the fact that the axes of the lakes are along the lines of outcrop of the rocks, and that the basins are excavated in the softer material.

Four other contributions, relating in one way or another to the glacial period, were read without much discussion; and the theories concerning the causes of the ice age were taken up. The Rev. Mr. Hill classed the theories as cosmical, terrestrial, and astronomical. The first class was not regarded as worthy of consideration, while terrestrial theories were as readily disposed of as being more or less unsatisfactory. Attention was directed especially to the theory of Dr. Croll, a combination of the precession and eccentricity theories. It was held that the part of Croll's explanations regarding fogs, deflection of currents, and the like, would support any or all theories alike. His conclusion was, that the alteration of currents and winds seemed to be the most powerful causes thus far suggested.

That part of Croll's theory regarding the greater eccentricity of the earth's orbit was attacked by Mr. W. F. Stanley in another paper. He could not conceive of the earth's initial temperature having been lower, or of the sun's heating power being less, and that therefore glaciation could not have depended upon such conditions. He regarded it as a local phenomenon, due to aërial and oceanic currents.

There was no session on the 30th, the day being given over to excursions. To the English geologists the occasion was a welcome one; and under the guidance of members of the geological survey of Canada, and of the local committee, they visited Ottawa, Ausable Chasm, Lake Memphremagog, Quebec, and various localities in the immediate vicinity of Montreal.

The prominent questions coming before the section on the 1st were those regarding archaean rocks. Professor Bonney opened the question with a lengthy paper upon these rocks in England, and made some comparisons with those of Canada. Dr. T. Sterry Hunt followed, treating of the eozoic rocks of North America. The paper was a *résumé* of some of Dr. Hunt's old work. As might have been expected, the very use of the word 'eozoic' was followed by some shaking of the head among the members; and, at the close of Dr. Hunt's reading, the use of the word was criticised as taking for granted a question which is still in dispute. The writer held, however, that his use of the word did not depend solely upon whether the supposed Eozoon canadense were the remains of a living organism, but upon the evidences of organic life having come into existence at or about the geological age referred to.

Professor Hall discussed the question at some length, and expressed the conviction that the solution of the problem lay in the study, not of large masses of rock, but in the study of junctures.

Every one was interested to hear what Sir William Dawson would say upon this question. He appeared to speak with some hesitation, due doubtless to the opposition to his well-known theories. He had but little to say; urging as a reason, that he was but poorly qualified to discuss the question from the standpoint from which it was being viewed, — namely, that of a chemical geologist. He said that he had spent his time in trying to find fossils in these rocks, and had got but little thanks for his labor. He would not enter the question in regard to Eozoon here. A co-laborer has the whole matter in hand now, and will soon publish all that is known. Major Powell was called upon, but limited himself to saying that we were not much disturbed by the question in the States, but were limiting ourselves to mapping the regions covered by these archean rocks.

The paper by Prof. J. D. Dana upon the southward ending of a great synclinal in the Taconic range was read by Professor Brewer, and elicited some very heated and severe protests on the part of Dr. Hunt. He insisted that the structure referred to was known twenty years ago, that the metamorphosis of sedimentary beds assumed by Professor Dana was untenable, and that there was no vestige of a proof of such a thing. Professor Brewer replied in behalf of Professor Dana, that recent and thorough work had been done in the region referred to, and that nothing was stated upon assumption. Major Powell was astounded that Dr. Hunt should speak as he did, if the structure was as represented; and he called upon him to either give his reasons for such statements, or to retract them, for the only way to attack such a question was to attack the structure. Professor Hall opposed Dr. Hunt's position, and vouched for the structure as represented; and Dr. Selwyn spoke of the existence in British Columbia of crystalline rocks in the carboniferous. Mr. Topley of the English survey then spoke of the general acceptance, by the various European surveys, of the theory of the change of sedimentary to crystalline rocks; and here the discussion of the archean rocks ended.

Members of the English survey exhibited maps colored so as to represent the solid geology; and others, of the same places showing the geology as it is actually seen upon the surface, that is, including the drift. This latter was regarded as valuable in connection with questions of water supply. Doubt was expressed, however, about the value of such surface maps save for local and temporary purposes, and it was suggested that some method be devised by which it would be possible to represent both solid and surface geology upon the same sheet.

The plan of Mr. Gilbert, of the U. S. geological survey, for a subject bibliography of North American geology, elicited some discussion. The section evidently felt a deeper interest in this paper than it was ready to express on so short a notice.

A brief account of his work upon the Jurassic mammals of America was given by Professor Marsh. Six years ago no Jurassic mammal was known; but five years ago they were found in Wyoming, and from one pocket alone from three to four hundred individuals have been taken, representing eight genera and twenty species.

Sir William Dawson spoke at some length upon the ancient land flora of the old and new world, calling attention to the striking correspondence found in countries widely separated.

Two paleontological papers by Mr. G. F. Mathews were spoken of in high terms, especially by the Canadian geologists; and the hope was expressed, that if, as had been suggested, one of the Canadian papers should be published in full by the association, the one upon the primitive Conocoryphean should be selected.

A paper by Prof. J. Milne, upon the earthquake phenomena in Japan, referred to the mechanical difficulties to be dealt with in his observations, and described a new earthquake house he has built upon large balls resting upon iron plates. Three hundred and eighty-seven earthquakes had been observed by him, eighty-seven per cent of which came from the sea.

Sir William Dawson then went over the leading facts worked out by Dr. Hall in his forthcoming geology of Palestine.

The last paper presented was by Mr. P. Hallett, and consisted of notes on Niagara Falls. For American geologists they contained nothing new.

It will be seen that nothing striking or new was presented to the section; indeed, some of the productions have been served up already a number of times and in various forms. But any thing different was hardly to be expected. The meeting was remarkable for bringing together workers in geology from every quarter of the globe. From Japan was Lyman, and a paper was read from Milne; from India were Blanford and Ball; from Australia were Blanford and Selwyn; from Africa was T. Rupert Jones; from Palestine was Professor Bauerman, and a paper was read from Hull; from Brazil was Branner; from England, Scotland, and Ireland, were the various members from those countries; from the States were Hall, Newberry, Marsh, Powell, and many others; while the Canadian workers were represented by Dawson, Selwyn, Whiteaves, and Adams.

PROCEEDINGS OF THE SECTION OF BIOLOGY.

In opening the biological section Thursday, Aug. 28, the president of that section, Prof. H. N. Moseley, delivered an address upon the physiology of deep-sea life. Well fitted as Professor Moseley is to discuss the subject of deep-sea life, on account of his long participation in its investigation during the voyage of the Challenger, his address was not only a critical and discriminating review of some of the later results arrived at by other observers and experimenters, but was supplemented by many valuable statements and suggestions of his own.

Mr. C. Spence Bate, of Plymouth, Eng., read a paper on the geographical distribution of the macrurous Crustacea, which embodied many important notes on form, color, habits, and habitats of different genera of these animals. In allusion to points mentioned by Mr. Bate, Professor Moseley said that deep-sea forms either had very large eyes or had no eyes, and that there must be a source of light in the deep sea; that source was phosphorescence, but its light must be very dim. The question was still unanswered, whether the larvae of deep-sea crustacea were found at the surface, as are the larvae of other crustaceans, and had to descend two or three miles through the ocean to reach their feeding grounds as adults.

Prof. W. J. Sollas, of Dublin, read a long paper on the origin of fresh-water faunas. The main difficulties in the way of most marine animals becoming inhabitants of fresh water were considered under three different heads: first, the time requisite for the animals to adapt themselves to the new medium; second, the greater severity of climate experienced by animals in fresh water than in salt water; and, third, the inability of marine animals with free-swimming larval stages to enter the mouths of fresh-water streams, or to breed in flowing streams if they gained access to them. In regard to climate, it is a fact that many marine forms become fresh-water ones as we approach the tropics. But severity of the climate of fresh water is not alone sufficient to account for the absence from it of many families well represented in marine faunas. Professor Sollas had prepared an extensive table, comparing by orders and by families the animals of fresh with those of salt water, and finds as a rule, with some exceptions, which he accounts for by peculiarities of life-history, that fresh-water animals carry their ova in or about them during the earlier stages of development, or they develop by buds or statoblasts. Some marine forms have passed from the ocean into marshes, and thence into streams; while other forms, especially during earlier geological times, owe their transfer into fresh water to the changing of marine into lacustrine areas. Professor Sollas reviewed some of the relations which the origin of certain fresh-water forms have to geological periods and changes, and considered some of the causes of modification of form and of prolongation of embryonic life of marine animals.

On the succeeding days a few papers upon the geographical distribution of animals were presented. Dr. G. E. Dobson pointed out that many of the most characteristic species of the chiropterous fauna of Australia have their nearest allies not in the Oriental but in the Ethiopian region, and instanced the presence of species of certain genera of bats in Madagascar and Australia which were poorly or not at all represented in India. We are therefore obliged, for this and other reasons, to suppose, that, at a comparatively recent period, a chain of islands connected Madagascar with Australia; the islands being sufficiently far apart to prevent the distribution of terrestrial mammals, yet near enough to permit the occasional passage of flying species. Later, a temporary connection of a similar kind probably extended between Madagascar and India. Treating geographical distribution of animals in a less general manner, was a paper by Mr. Howard Saunders on the geographical distribution of the Laridae (the gulls and terns), with special reference to Canadian species.

As to the distribution of plants, Professor Asa Gray, in his remarks on the characteristic features of North-American vegetation, called attention to the resemblances and the differences between the flora of North America and that of Europe, and to the causes of these resemblances and differences. The similarity of the trees of the Atlantic border to those of Europe was alluded to, and its cause discussed; and mention was made of the pleasure which the European botanist would experience in finding, in the new world, plants growing wild which are cultivated in the gardens of Europe. Among these are species of Rhododendron, Cypripedium, and Coreopsis. Turning to the differences between the flora of Europe and America, the wealth of species of trees and shrubs in the latter country was illustrated by numerical comparisons of the species of oaks and of many other trees in Canada with those found in England. Besides the far more numerous kinds of leguminous trees, and the remarkable wealth in species of Compositae which is noticeable in America, there are many tropical plants which extend northward into the United States. Such are various trees, and Sarracenia, Passiflora, Tillandsia, and numerous other herbaceous plants. After discussion of the part which the ice of the glacial period played in the distribution of plants over Europe and North America, Professor Gray reviewed the characteristics of the flora of the middle and western portions of North America. This paper was one of the few which the general committee voted to print in full in its proceedings.

Remotely connected as it is with the question of the distribution of trees in the United States, attention may be called to the Jesup collection in the New-York museum of natural history, which was briefly described in a paper by Prof. A. S. Bickmore. This collection, besides illustrating the wood, bark, leaves, and other parts of the trees of the United States, by dried specimens or by figures, inside the museum, is supplemented by having the trees about the museum numbered to correspond with the specimens, so that immediate reference can be made to the museum by any one who wishes to learn more about a tree seen in the park.

On the question of the affinities of different groups of animals, as shown by their anatomy or development, several papers of importance were read; but of the greatest value was the announcement made in a brief telegram from Professor Liversedge, in Australia, announcing that Mr. W. H. Caldwell, who is in Australia in order to study the development of some of the curious animals found there, had discovered that the Monotremata are oviparous, and that the egg is meroblastic. No statement was given in the telegram as to whether the facts were determined as regards Ornithorhynchus or Echidna; but the main points of interest are the discovery of the oviparous habits of a mammal, and the meroblastic development of its egg, as in reptiles, since the eggs of mammals are regularly holoblastic. This shows that we must turn to the reptiles for the ancestors of the mammals.

Prof. O. C. Marsh read a paper on the classification and affinities of dinosaurian reptiles. It was replete with facts derived from the large amount of material which has been accumulated within the last half-dozen years. Three orders were recognizable in the herbivorous, and one order in the carnivorous dinosaurs. In the carnivorous groups we have forms with greatly enlarged pelvis, and animals that sat down. One of them which was found the past year, Ceratosaurus, exhibits new characters for a dinosaur. The vertebrae are smooth in front and concave behind. The pelvis is made up of three coössified bones, as it is in birds, and not of separate bones as in Archaeopteryx and in other dinosaurs. Ceratosaurus also agreed with adult birds in having the three metatarsal bones coössified. The dinosaurs are thus shown to be very closely related to birds; and, in answer to a question, Professor Marsh called attention to the correspondence between the double sternum of larger dinosaurs and the ossification of the sternum from two centres in young birds.

Prof. A. Milnes Marshall showed, in a paper on the mutual relation of the recent groups of echinoderms, that Carpenter was correct in regarding the central capsule with its radiating axial cords in Comatula as the central nervous system, while the subepithelial bands, which Ludwig and others have regarded to be the true nervous system, are, in reality, nervous in character, but of subordinate importance. Professor Marshall has proved these points by a series of conclusive experiments, which he conducted at Naples upon the living animals. In regard to the homologues of the parts of the nervous system of crinoids in other echinoderms, Professor Marshall says, "I consider that in crinoids the subepithelial bands most certainly are homologous with the radial or ambulacral nerves of a star-fish; and I consider that they represent a part of a continuous nerve-sheath which has retained permanently its primitive continuity with the epidermis. The axial cords, some of the branches of which can be traced into extremely close proximity with the subepithelial bands, I regard as portions of the antambulacral nerve-sheath which, like the radial cords of echinids, ophiurids, and holothurids, have lost the primitive position, and shifted into or through the dermis."

Mr. William Bateson, in a paper read by the secretary, upon the presence in the Enteropneusta of a structure comparable with the notochord of the Chordata, made some interesting comparisons in regard to the relative positions of the nervous system the digestive tract, and the supposed notochord in Balanoglossus and in vertebrates. He added further comparisons between this animal and the vertebrates, and between its larva 'Tornaria' and the larvae of echinoderms.

Among anatomical papers containing facts which have a less general bearing on theories of animal relationship may be mentioned, as of especial interest or importance, the following: Professor Moseley described the position and minute structure, as determined from sections, of the eyes and other sense organs in the shells of the Chitonidae. The same gentleman showed that the arrangement of the feathers in groups of three each in the dodo had a close connection with the filoplumae, or thread-feathers, one of which is found at each side of the feathers of birds of the dove-family, near which the dodo is placed. Earlier in the development of the doves' feathers, the filiplumae are larger, relative to the size of the other feathers; and this condition resembles still more the condition found in the dodo. Prof. R. Ramsey Wright described the histological structure of certain sensory organs of the skin of the horned-pout (Amiurus), and discussed the function of the air-bladder in the same fish, and the relation of its air-bladder to the auditory apparatus. Prof. J. Struthers, of Aberdeen, described the rudimentary hind limb of the hump-backed whale (Megaptera longimana), and compared its thigh-bone with the same bone in other cetaceans. In a hump-backed whale forty feet long; the thigh-bone was entirely cartilaginous, being on one side four inches, and on the other five and a half inches long.

As a contribution to our knowledge of curious habits of plants, Prof. H. N. Moseley communicated some observations on the trapping of young fish by Utricularia vulgaris, a water-weed. After sketching and describing the bladders of this plant, which have been known for a long time to capture small crustacea, the speaker said that it had been lately discovered that these bladders also entrap young fishes. The fish, usually caught by the tail, is often, on account of its struggling, gradually drawn almost entirely into the bladder.

At the beginning of the session on Friday the 29th, reports of several committees were presented, among them that on the Naples zoölogical station. In this report, after mention of the various undertakings of the station, and of the work accomplished by Mr. A. G. Bourne and by Prof. A. Milnes Marshall, the two late occupants of the British association table at the station, the committee recommended that the association renew its grant for the table, and increase the amount paid to a hundred pounds (instead of eighty and ninety pounds as in previous years). This recommendation was adopted by the association.

RECENT PROCEEDINGS OF SCIENTIFIC SOCIETIES.

Trenton natural-history society.

Aug. 12.—Dr. C. C. Abbott continued his remarks on the life-history of Scaphiopus solitarius, the spade-footed hermit-toad. The adult toads appeared in April, when they presumably did not deposit eggs, and in June, on the 26th of which month eggs were laid. These hatched by July 3, and six days later the tadpoles showed small hind-legs. In thirty-one days after laying the eggs, the young resembled the adults in all except size, and, when placed on wet sand, at once buried themselves. Before leaving the water, they tend to prey upon each other. ——Dr. A. C. Stokes remarked in reference to a captive Tarantula arenicola, that, having been deprived of building-materials, she erected a wall of earth and small pebbles, and on July 8 formed an irregular dome over the burrow, leaving a central opening, which she closed with web. July 28 she destroyed the dome, and emerged with her abdomen thickly covered with young spiders. Although the latter were presumably only ten days old, they were becoming venturesome. They swarmed over the mother; but, when trespassing on her face, they were swept off by a stroke of her leg, and allowed to run back to her body. Occasionally they climbed up the tube, and wandered about the surface. Formerly the mother was very timid, retreating into the burrow when the observer arrived at a point twelve feet from the entrance; but, after the young appeared, she permitted the observer to approach and move about at pleasure. She also accepted food from the hand. She took a fly, and remained at the surface sucking its juices. The fly was removed from her mandibles by forceps, and a black ant offered; but it was thrown away as she throws away the excavated earth. A full account of the habits of this spider will be found in *Science*, iv. 114.

Cincinnati society of natural history.

Aug. 5.—Mr. U. P. James presented a paper on conodonts and fossil annelid jaws from the lower Silurian of south-western Ohio. The only annelid heretofore noticed from these rocks in Ohio is that described by Prof. G. B. Grinnell in *Amer. journ. sc.*, September, 1877, under the name of Nereidavus varians, and referred to the jaw apparatus of an annelid. Mr. James has discovered other forms which are similar in character. They occur as small, dark, shining objects, varied in form, and detached from each other, of a glossy black tint, though changed by weathering to a rusty red. They are composed of chitinous matter, and undergo no change in nitric acid. Mr. James has identified some of the forms with species described by Mr. G. J. Hinde. Conodonts were first noticed by Pander, in 1856, and have been referred to as fish-teeth. Though their zoölogical relations cannot be finally determined until found in position, the best authorities agree in thinking them the lingual armature of large naked mollusks. Dr. Newberry has described conodonts from the Cleveland shale of the Waverly group in Ohio, and Mr. Hinde figures forms from the Silurian of Canada, and Devonian of the United States. They are now identified from the Cincinnati group of Ohio, some of the forms being identical with those from Canada and England.——Mr. Charles Dury stated that he thought the Oswego and black bass (Micropterus dolemieu and M. nigricans) were but forms of one species. The black bass is always found in swift-running streams, while the Oswego bass inhabits sluggish waters, ponds, and lakes. The Oswego bass is of a much larger size, lighter color, and has a larger mouth, than the black bass: hence the name of the white or large-mouthed bass. Ross Lake, an artificial pond near Cincinnati of about forty acres, was stocked a few years ago with black bass. It now swarms with the other form. Though many specimens of M. dolemieu have been taken, not a single M. nigricans has been caught, as far as known. Other instances were cited in which the large-mouthed species had appeared in ponds which were stocked with the small-mouthed form. Mr. Dury concluded that the Oswego bass is a variety of the black species, due to a difference in habit and to a superabundance of food. Dr. W. A. Dun said that he had caught the large-mouthed species in the Kanawha River, under the falls, though he thought that Mr. Dury's conclusion was in the main correct. Dr. D. S. Young agreed with Mr. Dury. He said, that, as far as color was concerned, he had observed that to vary with the season. The fish were of a lighter color in summer and in warm water than in winter and in cool water. He had caught the large-mouthed bass in rapid-flowing streams, under circumstances which showed that they had probably escaped from overflowed ponds or dams.

NOTES AND NEWS.

THE McGill university convocation conferred upon the following members of the British association, at its final meeting in Montreal, the honorary degree of LL.D.: Lord Rayleigh, the Governor-general of Canada, Sir Lyon Playfair, Sir William Thomson, Professor Bonney, Professor Frankland, Captain Douglas Galton, A. G. Vernon Harcourt, Sir Henry E. Roscoe, Professor Blanford, Professor Moseley, General Lefroy, Sir Richard Temple, Sir Frederick Bramwell, Dr. E. B. Tylor; also upon the president of Toronto university, Professor Daniel Wilson, Professor Asa Gray of Harvard, and Professor James Hall, New York state geologist.

—At the recent meeting of the British association in Montreal, the general committee appropriated to scientific purposes certain grants of money for the ensuing year, amounting in all to £1,515. In the department of *mathematics and physics*, the largest sum (£100) is devoted to the calculation of mathematical tables; £70 is to be used in the investigations on meteoric dust; synoptic charts of the Indian ocean and meteorological observations on Ben Nevis each receive £50; one-half this sum is devoted to meteorological observations near Chepster; £20 is given for the study of solar radiation, and £10 for the reduction of tidal observations in the English channel. In *chemistry*, £25 is devoted to vapor pressures and refractive indices of salt solutions, £20 to physical constants of solutions, and £5 to chemical nomenclature. In *geology*, for volcanic phenomena of Vesuvius, £25; for the Raygill fissure, £15; for earthquake phenomena of Japan, £75; for fossil Phyllopoda of the British paleozoic rocks, £25; for fossil plants of British tertiary and secondary beds, £50; for geological record, £50; for erosion of sea-coasts in England, £10; for circulation of underground waters in England, £10. In *biology*, for a table at the zoölogical station at Naples, £100; for a record of zoölogical literature, £100; for observations on the migration of birds, at light-houses and light-ships in England, £30; for an exploration of Kilimanjaro and the adjoining mountains of equatorial Africa, £25; for recent Polyzoa, £10; for the marine biological station at Granton, Scotland, £100; for marine biological stations on the coast of the United Kingdom, £150. In *geography*, appropriations were made for the exploration of New Guinea by Mr. Forbes to the amount of £200; and the exploration of Mount Roraima, in Guiana, by im Thurn, £100. In the department of *mechanics*, £5 was devoted to patent legislation. In *anthropology*, £50 is to be used for the investigation of characteristics, physical and otherwise, of the north-western tribes of Canada; and £10 for the study of the physical characteristics of races in the British isles.

—The Annuaire of the bureau of longitudes of Paris for 1884 (p. 847) contains M. Janssen's report on the French expedition to observe the total solar eclipse of 1883, May 6. The text of this report has been previously printed in the *Comptes rendus*; and it is referred to here principally to call attention to the photograph of the corona given on p. 852, which did not accompany the report in the first instance. This photograph was made with a camera, mounted equatorially, which had an objective of eight inches aperture, and a focal length of about forty-seven inches. The exposure was over five minutes.

The diameter of the sun is about three-eighths of an inch, and the coronal outline is in general quite thirty minutes from the sun's *limb*. Streamers extend more than twice this distance from the limb.

There is no great amount of detail in this picture, as was to be expected; and we shall look for the publication of the photographs of shorter exposure with interest.

One important fact is stated by M. Janssen; to wit, that, so far as his photographs have been examined, they show no trace of an intra-mercurial planet.

— Mr. Cochery, the French minister of posts and telegraphs, according to the *Science monthly*, reports to the French academy of sciences, that there were in France, during the first half of the year 1883 (from the beginning of January to the end of June), the following strokes of lightning. In January there was a stroke injuring a man who carried an open umbrella with metal ribs. In February there were no strokes at all. In March there were four strokes, damaging unprotected buildings and a high oak-tree. In April there were only four strokes, injuring several persons, some poplar trees, a weathercock, a bell-tower, and an isolated building. In May there were twenty-eight strokes, killing two men, seven cattle, three horses, and injuring several persons and two horses, as well as numerous trees and houses. The trees were oaks, chestnuts, poplars; and several of the strokes attacked the chimneys of the houses. It is notable that a gilt wooden figure of Christ in front of the church of Bonsecours (Seine inférieure) was struck, although the church has a lightning-rod on it. During the month of June the total number of strokes largely increased; there being no less than a hundred and thirteen, or from three to four a day. The daily number varied during the month, but was, if any thing, larger at the end than at the beginning of the month. Seven men were killed. About forty persons — men, women, and children — were injured. Some seventy animals were killed, including fifty sheep and a dog. Many trees, oaks, poplars, elms, firs, were struck. A common object struck is the bell of some church, the chimney of some house, or the weathercock of a barn. Some of the strokes were received by the lightning-rods of buildings, and did no harm, except, perhaps, fusing the point of the rod. On the other hand, several accidents to buildings, and in one case death to a horse, occurred within a comparatively short distance of a lightning-rod (from fifty to eighty metres). Isolated trees, and animals under them, appeared to have suffered most. Rain and hail accompanied most of the storms.

— Mr. Frederick John Smith writes to the *Electrical review* as follows: —

"Considerable trouble has been felt by those who are engaged in practical problems connected with secondary batteries, arising from imperfections in the cells for holding the dilute acid, and also from the fact that the plates of a charged secondary battery cannot be lifted out of the liquid, in order that any required area may be exposed to the action of the acid, without the rest of the reduced lead on the kathode plates being at once acted on by the oxygen of the air. To meet these difficulties, I have carried out the following methods: The cells are made of common pottery-ware about two centimetres thick. All sharp corners should be avoided in the moulding of the cells, because they do not stand the process of cooling well, while rounded corners seldom crack during cooling. These rough porous cells are warmed slowly in an oven, to such a temperature that paraffine-wax melts easily when rubbed against them. The cells, on being removed from the oven, are partly filled with paraffine-wax: this is made to run well over the whole inner surface of the cell. As soon as the wax begins to set, it is poured out, and the cell is put away to cool. A cell so made stands acid well; and the dilute acid does not creep up the sides of the cell, as it does in the common glazed cell. Another method, used at an earlier date than the one just mentioned, was to make deal boxes of the size required, and place inside them card-boxes (held out by sand), so that there was a space of about one centimetre between them. This space was filled with common paraffine-wax; then, the card-box being removed, a perfect lining of wax was left. This method is more costly than the last, but has the advantage of greater strength. The test of two years' constant use has shown that both these forms of secondary battery cells are both practical and lasting. When using a secondary battery in the laboratory, it would sometimes be convenient to be able to expose only some part of the plates to the action of the dilute acid; but, as things now are, this cannot be done without the part of the plates which are lifted out being at once acted on by the oxygen of the air. To prevent this action taking place, the plates are drawn out of the liquid into the vapor of benzol (after several experiments with different gases, this appeared to answer well, and to be easily managed). By this means the injurious action mentioned is prevented, and any required amount of surface of plate may be exposed to the action of the dilute acid."

— The *Revue scientifique* states, that, notwithstanding the ravages caused by the Phylloxera, France is the country which furnishes commerce with the greatest quantity of wine. Of the hundred and fifteen millions of hectolitres produced by Europe in 1881, France furnished thirty-four millions; while the average from Italy, Spain, and Austrian Hungary was only from twenty to twenty-five millions, and that of Germany, Portugal, Turkey, Greece, Roumania, and Switzerland, varied from four millions in Portugal, to one million in Switzerland. At present France supplies its lack of harvest by importing wines which it again exports, doctored, and mixed with its own. It receives wines especially from Spain, Italy, Portugal, and Greece. It treats the settlings, the residuum of the native harvest, with sugar, alcohol, and water, and thus makes wines known as the 'second vat.' It also makes wine of raisins received from neighboring high countries and from Syria. To the raisins, softened in water, sugar and alcohol are added, one kilogram of raisins yielding from three to four litres of a harmless wine. This manufacture is carried on especially at Marseilles, at Cette, at Bordeaux, and at Bercy. The importation of raisins into France amounts to seventy thousand tons, representing thirty eight million francs: these raisins give about three million hectolitres of wine. The wines of the second vat amount to about the same quantity.

—Victor Giraud writes from Karema in good health. He had spent a month on Lake Bangweolo, where several errors of the charts of Livingstone were corrected, among others the position of the Luapulu River, which really comes out of the south-west part of the lake, instead of the north-west. This part of the work was undertaken with eight men, the remainder of the caravan waiting for Giraud near Kazembe. Harassed by the natives, their boat was finally abandoned near the cataract of Mombottuta. At ten days' march they reached the chief of the Muaumi, who detained them in semi-captivity two months. Finally escaping, they crossed Itahua, and reached Tanganyika and Karema by the 14th of February last. Giraud intended to remain there about a month, then to return to M'para, and attempt to reach Leopoldville by traversing Marungu and the Lualaba on about the 6th parallel.

— Bishop Levinhac has left Tabora, and is momentarily expected at Zanzibar. The stations under his supervision were flourishing at last accounts, as were those of the Pères du Saint-Esprit.

— 'Bird nomenclature of the Chippewa Indians' is the heading of an instructive linguistic article inserted by W. W. Cooke in the July number of the *Auk*. The Ojibwē names of one hundred and twenty-six birds, most of them with their etymologies, are enumerated in this paper; and it may be safely said that only a naturalist can obtain the Indian equivalents of so many species with so much accuracy as we see it done by Cooke. These Indians give names to all winter residents, since at that time bird-life is so scarce that each one is accurately noticed; but of summer residents they know with distinctness only those hunted for food.

As stated by Cooke, nearly one-half of the bird names given by Bishop Baraga in his celebrated Ojibwē dictionary have wrong definitions. If true, this will go to show, that, to take down correctly the Indian equivalents for objects of nature, the collector has to be a linguist and a naturalist at the same time; but it is by no means certain that the Indian names of birds and other animals do not sometimes shift from one object to another similar one. Ridgway, Cope, Gabb, and others have paid considerable attention to the gathering of Indian terms of natural history; and it is desirable that other naturalists follow their example, giving the etymology of each name, if traceable.

— Many local names occurring along the Mosel and the Middle Rhine have, through their quaint and foreign sounds, proved attractive to historians and linguists. Hubert Marjan, their most recent investigator, has just published the fourth instalment of his critical researches (Rheinische ortsnamen, 39 p.) on the subject, in which he follows the only true method to disclose the origin of names, which is the historic one. The early orthographies of names, as found in Roman authors and in the more ancient mediaeval parchments, necessarily come nearer to the original forms than the name-forms we use to-day: hence Marjan bases his conclusions upon the earlier forms, and in the majority of instances his results meet our approval. The most ancient topographic names of these parts are Celtic; but the names of Low-Latin and Romance origin far exceed the Celtic ones in number, the German names being late additions. Thus Nehren is derived from *nucaria* (*silva*), 'walnut-grove;' Tholey from *tilietum*, 'linden-grove;' Kärmeten from *carpinetum*, 'horn-beech grove;' Zons from *uncia*, 'agricultural field;' Ülpenich from *Ulpius*, a man's name. In the mountainous tracts of the Hunsrück, Maifeld, and Eifel, our author discovers a considerable sprinkling of Slavic names, but neglects to follow up their etymons through all the eight or ten Slavic dialects known to us. The existence of Slavic names in these western countries is explained by the historic fact, that, after a Gothic war, the emperor Constantinus settled three hundred thousand Sarmatae in various parts of the Roman dominions, a part of which can be historically traced to the Hunsrück and to the plateau of Langres in France (about A.D. 334). Prof. A. Bacmeister had previously (1870) attempted to trace local names of Bavaria and eastern Würtemberg to a Slavic origin.

— We reproduce from *La Nature* a cut illustrating an experiment which shows the pressure of the air most markedly. A thin strip of board is rested on the edge of a table, its inner end being covered by a

sheet of paper, as shown. When arranged in this manner, it will be found that a sharp blow may be given the board, without effect, even if it would fall of its own weight without the paper.

— At a meeting of the Royal astronomical society on June 13, Mr. Ranyard read a paper on the cause of blurred patches in instantaneous photographs of the sun. If the image of a bright star in a reflecting telescope is observed out of focus, ripples of light may be seen passing across the bright disk, which is really an image of the speculum, with the flat projected on its centre. That these ripples are due to the unequal refraction of heated air-currents, Mr. Ranyard showed by placing a hot iron in the tube of the telescope, which increases the distinctness of the ripples, as well as the velocity with which they move across the image. In the image of a uniform bright disk, their effect is to give rise to areas of greater and less brightness, which float across the field as the heated air rises. This was proved by means of instantaneous photographs of the sun, taken with a heated iron in the mouth of the telescope, and when the sun was near to the heated roof of a house.

—An announcement is made in the *English mechanic*, that oil-bearing strata exist in the neighborhood of Sibi, southern Afghanistan; and the government have determined to procure the necessary machinery for boring-operations, which, it is said, will be commenced next winter.

—Mr. C. L. Prince of Crowborough has presented to the Royal astronomical society a great rarity in the shape of a copy of Sherburne's poetical translation of Marcus Manilius, 1675. The volume is valuable for the extensive list of oriental astronomers it contains, and as an English translation of Manilius's Astronomicon poeticon. Mr. Knobel said that for six years he had searched all the booksellers' catalogues without finding it. The library of the Royal observatory, Greenwich, came into possession of a copy by purchase four or five months ago, and it may seem not a little remarkable that two copies of so rare a work should come to light almost at the same time.

—A full list of the papers at the International conference on education, in connection with the International health exhibition, appeared in *Nature* for July 10.

—Number xiii. of the signal-service professional papers, recently issued, contains the results of an extended investigation by Professor William Ferrel on the 'Temperature of the atmosphere and earth's surface.' This is Mr. Ferrel's first memoir completed since his engagement under the chief signal-officer: it is characterized by the same comprehensive mathematical treatment of physical problems that marked the 'Meteorological researches' which he undertook a few years ago for the coast-survey. The broad subject of meteorological temperature is arranged under four headings,—first, the relative distribution of solar radiation on the earth's surface (the mean vertical intensity of solar radiation for one day at the top of the atmosphere is here tabulated for twenty-four epochs in the year, and for every ten degrees of latitude in the northern hemisphere); second, the conditions determining the relations between the intensities of solar radiation and the resulting temperatures, in which the diathermance of the atmosphere is considered; third, the general subject of actinometry, in which two series of experiments give the mean solar constant as 2.255 and 1.991, and from these, compared with others, the value 2.2 is taken as most probable (it is here concluded that stellar heat is insignificant, and that there is no sensible temperature of space; fourth, the distribution of temperature on the earth's surface, and its variations, where, among many conclusions, there may be mentioned the determination of —100° C. as the approximate mean temperature of the earth without an atmosphere; 0.213 as the share of dark heat radiated vertically from the earth's surface, which escapes through the atmosphere into space; and the difference between mean equatorial and polar temperatures on a dry-land earth at considerably more than 115° C., ocean-currents being chiefly responsible for diminution of this extreme condition.

—The English bark Churchstow, Capt. Adams, reports that in a voyage to Columbo, Ceylon, she fell in with large quantities of pumice-stone, Feb. 29, 1884, in latitude 18° south, longitude 73° east. The pumice-stone was partly covered with barnacles.

—It seems that Mr. Cailletet has perfected his method for liquefying oxygen; since this body may be obtained in sufficiently large quantities to appear in the form of a colorless liquid, very volatile, and much resembling liquefied sulphurous acid. The author began by liquefying ethylene by the aid of solid carbonic acid and pressure. By means of this he liquefied formene; and, by the cold produced during the evaporization of the formene, oxygen was finally liquefied.

—*Nature* states that the educational statistics of Japan for the past year show that the number of common schools throughout the country is 29,081, being an increase of 339 as compared with the preceding year; while the number of scholars is 3,004,137, an increase of 396,960; and the number of teachers is 84,765, being an increase of 8,147.

—Miss Amelia Edwards, the honorary secretary of the 'Egypt exploration fund,' has made a communication in the *Academy* about the remains of the statue of Ramses II., found by Mr. Petrie at Tanis. These remains are of red granite. The statue of Ramses II., the contemporary of Moses, was overturned by one of his successors, Sheshank III. By an exact examination and photography of all which was found, Mr. Petrie has come to the conclusion that the statue must have had a height of a hundred and fifteen feet, and thus exceeded all the monuments of that sort hitherto known. The great toe of the statue has a circumference of a foot and a half.

—From a communication of Dr. S. Glasenapp, of the Imperial university at St. Petersburg, to the Russian newspapers, there are in Russia, as we learn from *Nature*, the following private observatories: one at Pervin, near Torjok, in the government of Tver, belonging to Gen. Maievsky; another at Bunakovka, in the government of Kharkoff, belonging to Prince Liven; and one at Odessa, belonging to Mr. Gildesheim. A Polish gentleman, Mr. Wuczihowski, is building a private observatory at Belkave, near Breslau; and a Russian gentleman, W. P. Engelhardt, has a fine observatory at Dresden, equipped with an excellent twelve-inch refractor and a large spectroscope, as well as a selection of the best physical instruments.

—Professor Milne of Japan, says the *Athenaeum*, has established in the Iakashima coal-mine, near Nagasaki, an underground, or, as he prefers to call it, a cetachthonic, observatory. This colliery is worked for some considerable distance under the sea; and it is purposed to establish a regular system of observations on temperature and pressure, and on the tides, earth-tremors, and the escape of gas, carefully noting if any connection exists between them, and establishing a comparison between surface and subterranean phenomena.

—An interesting statistical statement on the use of shorthand-writing has been issued by the U. S.

bureau of education as the second of its series of circulars for 1884, accompanied by a bibliography of the subject so far as American and English authors are concerned, containing about fourteen hundred titles. More than as many German works are known, and publications are abundant in other countries. A comparative view of a hundred and twelve alphabets from 1602 to 1882 is given on a single sheet. The use of shorthand has largely increased in the United States within the past five years. In Washington the management of some of our scientific bureaus, on their present extended scale, would be almost impossible without it. Certainly the efficiency of bureau service is vastly increased by its use.

— The April number of *Memorie della Società degli spettroscopisti italiani* contains a paper by Dr. J. Hilfiker, entitled "Première étude sur les observations du diamètre du soleil faites à l'Observatoire de Neuchâtel de 1862 à 1883," in which these observations are discussed with reference to a supposed variation in the apparent angular diameter of the sun, due to or coincident with the periodicity of the solar spots. The evidence seems to point toward the coincidence of the lesser diameters with the epoch of maximum spottedness of the sun's surface.

— The rain-band spectroscope has a rival in the scintillation of the stars, as shown by the studies of Mr. Ch. Montigny (*Bull. acad. roy. Belg.*, April, 1884). He finds that blue scintillations are more frequent on the approach of rain, and considers this the result of the greater quantity of water in the upper atmosphere. On the basis of the recent continued diminution of blue scintillations, the author ventures the prediction for Belgium, that the series of rainy years beginning with 1876 is now happily ended, and that a series of drier years is about to begin. The observations are of value, but the extension of their conclusions so far into the future does not seem justified.

— P. Tacchini has recently issued two reports of his studies in connection with rainfall, — Nota sulla osservazioni pluviometriche eseguite nelle stazioni forestali di Vallombrosa e di Cansiglio; Le febbri malariche e le meteore nella provincia di Roma: Roma, 1884. The first exhibits the results of rain-measures from 1872 to 1880 in open fields and under trees. The ratio of the latter to the former was from 0.74 to 0.64 under fir-trees, and 0.76 under beech-trees, and the ratio of loss increased in months of less rainfall. These ratios are, however, open to variation; as they depend on only a single gauge for the beech-trees, and on but two for the fir-trees.

The relation of malarial fevers to the weather in the province of Rome is a more extended study. A series of tables gives, first the number of cases of fever in the various parts of the province recorded for the third quarter of each of the twelve years from 1871 to 1882; then the percentages of fever to population, showing an average annual ratio of 6.077 per cent, falling to minima of 2.93 in 1878, and 2.49 in 1882, and reaching a maximum of 11.42 in 1879. These figures are next compared with rainfall, cloudiness, temperature, and winds; and there is found a clear correspondence between the fall of rain in March, April, and May, and the fevers of July, August, and September; an inverse relation between the cloudiness in June, July, and August, and the fevers of the third quarter; a minimum of fever with a maximum of sirocco winds; and certain indistinct relations of the other elements. All these results are well indicated in diagrams, as well as in tables. They give an increased value to the careful study of rainfall.

— The *Electrical review* states that the Jablochkoff electric candle, the pioneer of all arc-lighting on a practical scale, has ceased, after a period of more than five and a half years, to illuminate the Thames embankment, by reason of the termination of the contract with the Metropolitan board of works. The lights were put up in 1878 for a three-months' trial: consequently the works were not of a permanent character. Yet the lights, with the exception of a few occasional mishaps, have given general satisfaction. No more exposed position could have been selected for such a trial, and the successful working of the system under the circumstances still further proves its value. It is an open secret that the price (one and a half pence per hour per lamp) paid for the lights resulted in a considerable loss to the company. From the recent address of Sir J. Bazalgette at the opening meeting of the Institution of civil engineers this season, it appears that twice the illuminating-power is obtained on the embankment from the Jablochkoff lights as could have been obtained from gas, if the same money were expended: in other words, the price should have been threepence per hour, as compared with the same light from gas.

— According to the *Revue scientifique*, June 21, a distinguished officer of the French army has studied the recently discovered coal-beds in Algeria, and who gives interesting details in the following passage from a letter to the Geographical society of Paris: —

It was at Bou-Saada, that I first heard of the coal reported to be found in Algeria. Coal is found all along the *oued* Bou-Saada, — a large river meandering through a country formed of almost vertical (80°) strata of reddish limestone. These strata lie parallel to the course of the river, so that it seems often to flow between two quite regular walls, whose summits are worn by the winter rains. This formation belongs, I believe, to the lower cretaceous. The traces of coal visible in the strongly eroded croppings which form the bed of the river are very slight (from .001 of a metre to .002 of a metre): they seem inseparable from the grayish-blue, sandy strata, which, at least in the exposed portions, are very small. This sandstone is hard and compact, often spangled with bright grains, which are, without doubt, iron pyrites. These are the first indications of the beds in question.

Mr. Pinard, who devoted himself to an examination of this bed, had shafts sunk at the places where he had determined the presence of croppings. There are three of these shafts, — two very near each other, 3.5 kilometres from the oasis toward the south, on the left bank of the *oued*; and the third is a half-kilometre farther. At my visit all were filled with water, so that I could study only what had been removed from them. The excavations consisted of sandstone like that mentioned, and of large pieces of marne, black sandstone, foliated, and enclosing thin strata of coal, which in some places measured .01 of a metre in thickness. Rumors of the officers stationed at Bou-Saada state that pieces of rather hard coal have been taken from the shafts, and that the stratum encountered was at times almost a metre thick. This coal, on breaking, is bright, compact, and of a good appearance, burns well, with a beautiful flame, and gives a light, brilliant coke.

— The Prussian minister of instruction has published an opinion on the overwork in schools through the medical deputation sent to him on the subject. The evil exists not only in the upper and middle classes of the high schools, but in the earlier school years. It is strongly recommended that pupils should not be received into the elementary schools until the completion of their seventh year, and not into the gymnasial sexta until their tenth year.

— The new German orthography, supposed to be more phonetic than the old, is to be made official next year in the Grand Duchy of Oldenburg.

— Two important geographical works are projected at St. Petersburg. One is, according to Professor Veniukoff, the preparation of a grand monograph on the physical geography of European Russia. Several members of the imperial geographical society have been constituted a committee to elaborate the project. The second is the preparation of a good general map of the same region, for the use of the public, to replace that issued by the society in 1863. The selection of matters to be omitted or retained is to be made by specialists, and approved by an editorial commission. The execution will be in the highest style of cartography.

The report on the unification of Russian geodetic and topographic work has been elaborated by the commission, and submitted to the general government for approval.

— Revoil has returned to Zanzibar from his explorations among the Somalis. Although prevented by a state of things resulting from the disturbances in the Sudan from carrying out his original plans, he made good use of his forced sojourn at Guelidi and on the Benadir coast. He devoted his attention to the archeology and natural history of this region, and has brought back valuable collections and notes on the resources and productions of the country.

— Ussagara mission station has been visited by famine due to drought. The Rev. Bloyet writes, that, notwithstanding this, the people about the station are well disposed.

— The work upon the canal between the gulfs of Corinth and Aegina is being energetically pushed, and another year will probably see it completed. Advantage will be taken of the vestiges of the canal begun by Nero. The trench will be a straight line, about six kilometres in length, including the basins at either end, and crossed by two bridges. The greatest height of the ridge to be pierced is about seventy metres. The completion of the canal will shorten the distance between the Adriatic ports and those of western Turkey, — Salonica, Constantinople, Smyrna, etc., nearly two hundred miles, and for vessels from the Atlantic about half as much, beside enabling them to escape much dangerous navigation. The tariff will be fixed at one franc per ton for vessels from the Adriatic, and half a franc for others. The monthly movement of tonnage is at present about 137,000 tons, mostly in small vessels, the local trade being extremely large. The contract for cutting the canal has been taken for about five million dollars, and there is no reason to suppose that this will be exceeded.

— The important question of a port of embarkation in south-eastern Brazil for the region about the lagoon or estuary known as Lagoa dos Patos has recently been discussed by the engineers Piazolles and Sichel. On the borders of the lagoon are the important colonies of Porto Allegre, Rio Grande, and Pelotas. By steamers of light draught communication is had with an extensive interior region containing a large population. The entrance to the lagoon, however, is composed of a shallow passage obstructed by shifting sands, where the bad weather of a day may obliterate the effect of dredgings during several months. The peninsula, which extends between the lagoon and the Atlantic, has been supposed to be of a sandy or porous nature, unsuitable for permanent works. Recent investigations by the above-mentioned gentlemen show, however, that this idea is erroneous, and that the foundation of the peninsula is a compact, hard clay, well suited for excavation. These engineers propose to select a favorable spot, where a large fresh-water lake exists, to dig out a small basin at the coast capable of containing several large vessels, and to connect it, by a canal deep enough to admit the largest ships, with the above-mentioned lake, which is to be dredged out to form an extensive basin or port. As the Lagoa dos Patos is too shallow to accommodate large ships, the freight is to be transferred, by a railway eighteen kilometres long, to the point where the light-draught vessels of the lagoon can be reached. The projectors ask only an authorization to make and maintain the works without subsidy or guaranty. The *Brazilian messenger* states that it is now practically certain that this important work will be carried out, thereby giving the colonists excellent facilities for commerce, the want of which has hitherto crippled the development of a rich and healthy region.

— The government of the Argentine Confederation, in the hope of obtaining water by artesian borings, has ordered an investigation of the geology of the San Luis district. Water is generally found only at a depth of one hundred and eighty feet. Potable water is usually reached at that depth; but at Upper Pencoso only salt water was found, though at a height of fifteen hundred feet above the sea, while at Cuijades the water is hot, reaching 75° F.